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THE TELEGRAPHIC JOURNAL AND

Electrical Review.

VOL. XV.—NO. 362.

SOME DYNAMO-ELECTRIC MACHINES.

So far as we can judge from the published reports of Professor Rowland's paper on "The Theory of the Dynamo-Electric Machine," read before the Philadelphia Congress of Electricians, we cannot see that he has added much, if anything, to the knowledge which electricians, at least in this country, had already gained. The paper, indeed, which seems to have been hastily compiled, is vague and somewhat unintelligible, as if the author had but a confused notion of what he wished to bring before the meeting. The discussion, however, in which Professor Silvanus Thompson took a prominent part, in a way which redounds greatly to his credit, was much more vigorous, and we can promise our readers something (in due course) well worth their perusal.

We notice that Professor Rowland expressed himself to the effect that the inner portion of the wire on a Gramme ring is useless. That this is not strictly the case will, we think, be readily admitted; but cannot someone devise a simple experiment, appealing to the perceptive faculties of all, to show to what extent the inner wire is active? This would be rendering a real service to electricians, in whose minds these doubts should not be allowed to remain.

Another machine which the Professor criticised rather severely, and one with which he was evidently not familiar, judging from the description and diagram, was that of Ball, the so-called "Unipolar." Professor Silvanus Thompson also passed comments the reverse of favourable upon this unfortunate dynamo, which appears fated to be looked upon as something quite uncanny. Yet the suspicions which cross the minds of both American and English electricians concerning this machine can only be due to its peculiar construction—they are certainly not based upon its performances. The "lines of force" theory does not, as Professor Rowland admitted, fit itself readily to this apparatus: and so without any further explanation the Ball machine is shelved. Our readers will remember the tests made in 1883 upon a Ball dynamo by the late Mr. Robert Sabine, and also others at the beginning of this year upon a later form of machine, which more than bore out the figures of the high efficiency obtained with the first. Perhaps the remarks of Prof. Nipher on this subject are more sensible and reasonable than those of any other scientist who took part in the discussion. This gentleman said: "It occurs to me to say a few words in regard to an unfortunate machine which has been referred to several times, and which I saw the other day for the first time. It has two armatures and only two pole pieces. I was as much astonished as anybody could be when I saw it; but when I was informed that it weighed less than 1,000 lbs. and maintained a high strength of current, I thought it worth while to investigate what the machine would do before we said too much about how it worked."

If we are not mistaken, one eminent Professor alluded to the Hochhausen machine last year as being both mechanically and electrically bad; and quite as eminent an electrician declared it to be—we believe at the same meeting—the most wonderful dynamo he had seen.

These discussions on theoretical points are very well in their way; but no man has a right to condemn a machine which to him seems at variance with his pet theory, unless, indeed, such an apparatus is palpably a fraud to the whole recognised scientific world. The best way of arriving at the comparative merits of the machines discussed in the Congress of Electricians at Philadelphia, will be to form a judgment from the reports of the examining committee, if time and other considerations allow the proposed tests to be properly carried out.

A NEW FORM OF THE BUNSEN PHOTOMETER.

By DR. HUGO KRÜSS.

BUNSEN'S oil-spot photometer is the most generally employed of all instruments for the measurement of light. As far back as 1843, when W. Th. Casselmann described this apparatus in detail for the first time, he proved experimentally that Bunsen's photometer admits of results considerably more accurate than the photometers of Rumford and Ritchie. Rumford's shadow photometer is no longer used for exact measurements on account of its great inaccuracy, and of that class of instruments which are derived from Bouguer's principle the only one to be considered is that of Foucault, which is extensively used in France.

Bunsen's photometer has hitherto met with far more general application in the gas manufacture than that of Foucault, and in electrotechnics also the Bunsen photometer is used in most cases. This preference is due to the reason, advanced by Casselmann as an advantage of this principle, that a difference in the colour of the two sources of light to be compared with each other is here far less detrimental.

The chief difficulty in the determination of the brightness of powerful arc lamps springs from the difference in colour of the electric light and of the gas, petroleum, or oil flame taken as a standard for comparison. The two surfaces of the photometer, whose brightness has to be compared, are coloured very differently by the two sources of light. Nearly all observers who use the Foucault photometer affirm that this unequal colouration of the two halves of the screen has a very disturbing influence in measuring the intensity of the electric light. If this is much less the case with the Bunsen photometer the reason is that it does not only depend on a comparison of the intensity of two surfaces illuminated by the two sources of light, but that we have a further gauge for the correct placement of the screen by attending to the equal distinctness of the margins of the oil spot against the unoiled paper on both sides of the screen.

Thus in working with the Bunsen photometer it is possible, after sufficient experience, entirely to overlook the different impressions of colour on the two illuminated sides, and by observing the margins indicated to obtain more accurate results than with the Foucault photometer, in spite of the different colourations.

This advantage seems to me decisive in the selection of a photometer. Still, the Bunsen instrument, notwithstanding various proposed modifications, suffers from a very palpable defect, which in many cases occasions Foucault's instrument to be preferred. In the latter the two illuminated surfaces to be compared with each other meet in a sharp line, whilst in Bunsen's photometer the result depends on the comparison of the two sides of one and the same paper screen. In all the arrangements based upon Bunsen's principle, one difficulty or inconvenience is always encountered. I, therefore, venture to point out a new method for reaching the object which may prove useful, though I do not deny the possibility of a still better solution of the question.

The construction of the Bunsen photometer depends on the circumstance that an oil spot upon paper, if seen by transmitted light, appears light on a dark ground, whilst by reflected light it is dark on a light ground. Hence we might infer that if the paper were illuminated equally strongly on both sides, the spot would appear neither light on a dark ground nor dark on a light ground, but would entirely disappear.

This would be in fact the case if the light falling upon the paper screen were resolved into two parts only—the one reflected and the other transmitted. If the screen is placed exactly in the middle between two sources of light of exactly equal intensity, observation shows that the oil spot does not disappear, but appears dark on a light ground. The incident light is resolved not into two but three parts, the third part being absorbed both by the oiled and the unoiled paper, but in a different proportion.

Thus there are to be noted three positions of the photometer screen—that in which the oil spot disappears to the right, that in which it disappears to the left, and that in which it is equally distinguishable from the unoiled paper on both sides. Mathematical investigation shows that from the mean position the correct proportion of the intensity of the two sources of light can be directly found according to the law of the decrease of brightness as the square of the distance. It may also be calculated from the geometrical mean of those relations of intensity corresponding to the two positions in which the oil spot disappears to the left and the right.

In the modification of the Bunsen photometer proposed by Desaga, and formerly much in use, only one of these three positions is employed, that, namely, in which the oil spot disappears on one side of the screen. If we call this side the front, a small gas flame is placed as an auxiliary source at a constant distance behind the screen, and is movable with it. The front of the screen is then turned successively to the two sources of light which are to be compared with each other, and brought each time to such a distance that the oil spot disappears.

This arrangement is applicable in the gas-manufacture where the sources of light differ little from each other, and are approximately alike in colour. In electro-technics it is inapplicable since sources of light have often to be compared which cannot be displaced with the paper screen, and, further, because the differences of colour are so great that the oil spot does not disappear at all. Work with this photometer is made more difficult by the necessity for employing an auxiliary light, which must be carefully kept constant.

The same objections must be raised against Zenger's differential photometer—a very fascinating instrument theoretically speaking. The paper screen is here fixed in the median line of a right angle formed by the two graduated rods on which the sources of light are moved. Here is the further difficulty that the sources of light which are to be measured have to be moved towards the screen in order to get the right distance. With many sources of light, especially electric arc lamps, this is impracticable. Besides, there would be required either an assistant or a complicated and consequently costly mechanical arrangement.

Rudorf's modification of Bunsen's photometer has been most extensively adopted. Here (fig. 1) the screen, P, with the oil spot, F, is placed in the median line of an angle of 140° formed by the two mirrors, S_1 and S_2. The observing eye, A, sees the images of both sides of the screen simultaneously in the two mirrors, S_1 and S_2, and can compare their respective intensity.

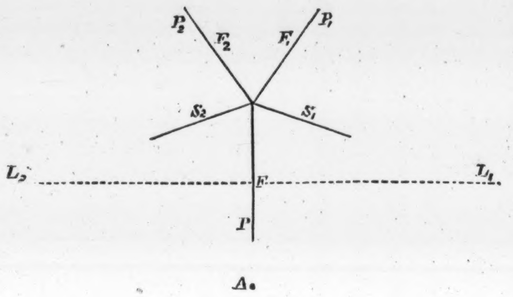

FIG. 1.

In this arrangement there is almost exclusively used that position only of the screen between the two sources of light, L_1 and L_2, in which the screen is equally illuminated on both sides, and in which, therefore, the images, F_1 and F_2, of the oil spot, F, appear equally dark on a light ground. The question may then be raised why, as the criterion of the complete disappearance of the oil spot is not used, this spot is not omitted and the Foucault photometer employed? The answer has been given above in the explanation how an increase of accuracy is obtained by observing the margins of the spot.

This modification of Rudorf's, with all its advantages, has two defects. If the distance from the source of light to the screen is not very large in proportion to the dimensions of the latter, the rays do not impinge approximately vertically upon all parts of the screen, and the illumination of the whole surface is not equal. A consequence is that the position of the screen is different as soon as the eye is turned a little out of the median line towards one side. The correct direction of the eye might, of course, be secured by looking through a stop with a moderately small aperture.

The second, and much more important defect, is that the two images F_1 and F_2 to be compared, lie at a great distance from each other. This defect is unavoidable, since, in consequence of the shadows which the mirrors throw upon the screen, the oil spot must be at a considerable distance from the apex of their planes. Even though a tolerable exactitude in observation may be attained by experience, the necessity of comparing two surfaces at a distance from each other is often a reason which deters observers from the use of the Bunsen photometer and leads them to prefer Foucault's instrument.

Some time ago I sought for and found a means of combining the great advantage of the Bunsen photometer, the use of the oil spot, with the advantage of Foucault's apparatus, the comparison of two surfaces in immediate contact with each other. Recently Hefner-Alteneck has made public a contrivance to the same end; this induced me to have my idea carried out and introduced in technical circles.

Hefner-Alteneck substitutes for the two mirrors placed behind the screen two prisms applied in front of the screen. An eye before them can thus perceive simultaneously both sides of the screen by means of refraction. The injurious space in the shadow of the two mirrors is thus avoided, the paper screen can be used immediately at the end of the line in which the two prisms meet, and the two illuminated fields, or their images, come thus in direct contact. In the figure with which Hefner-Alteneck illustrates his device, the optical axis of the photometer, certainly does not pass through the terminal point in question, but this would be easily effected by a displacement of the prisms.

An essential defect of Hefner-Alteneck's modification is, that the images of both sides of the screen are produced by refraction in a prism. In consequence, the images appear somewhat distorted and there arises a chromatic dispersion producing distinctly coloured margins even with the angle of the prism of near 30°, as shown in the figure. A very small angle can scarcely be selected, as the images then would be too little inclined towards each other and would consequently be difficult to observe.

In my arrangement I make, therefore, exclusive use of prisms, applying two reflection prisms, I and II. In the prolongation of the median plane in which the two prisms meet stands the screen, P. The angles formed by the planes of the prisms with each other are so selected that the rays which from all points of the screen between *a* and *b* fall vertically upon the plane, A_1, of the prism, I, are reflected at B_1, C_1, and A_1, and then issue out of the prism vertically to the plane, D_1. Similar is the course of the rays in the second prism, II. In front of the planes, D_1 and D_2, which meet at an acute angle, a tube can be applied of a length which can be varied according to the sight of the observer. At its end is a diaphragm with a small aperture by which the position of the observing eye is fixed in the plane of the screen.

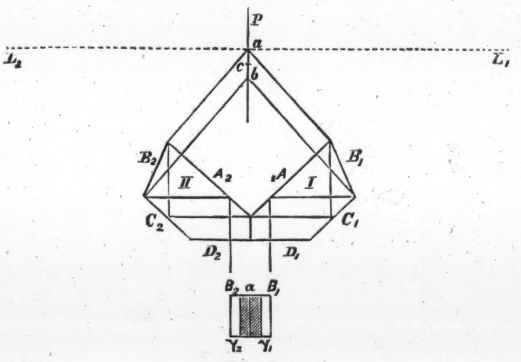

FIG. 2.

The eye then sees the field divided into two equal parts by the line, α, separating the two planes, D_1 and D_2; the right side is the image of the right side of the screen illuminated by the source of light, L_1; the left half is that of the left side of the screen illuminated by the second source of light, L_2. The image of *a* falls in the median line, α, and the images of *b* in the lateral margins of the field of vision, β_1 and β_2.

If the optical axis of the photometer is laid through the point, *a*, perpendicularly to the plane of the screen, and if the portion of the screen from *a* to *c* is oiled, γ_1 and γ_2 are the images of the margins of the oil spot. On both sides of the median line, α, if the photometer is set accurately, the illumination is perfectly equal, and this position is very convenient, as the surfaces to be compared are in immediate contact.

Experiments with this photometer have given very accurate results. By using a screen without the oil spot, or by displacing the screen so that the spot falls outside the effective space, *a*, *b*, the apparatus can be easily converted into a Foucault photometer, and the observer can convince himself that this apparatus gives decidedly less accurate results than Bunsen's oil-spot photometer.—*Repertorium der Physik.*

THE DISTRIBUTION OF ELECTRICAL ENERGY.

M. GAULARD, writing from the Turin Exhibition on the 15th October, sends the following communication to the editor of *La Lumière Electrique*:—"In the number of *La Lumière Electrique* for October 11th, you published a communication from Prof. Colombo on the experiments made at the Turin International Exhibition of Electricity by means of the secondary generators of Gaulard and Gibbs. This communication was preceded by an article in which M. Marcel Deprez seeks to show that the invention is not novel, that it has no scientific value, and that from an industrial point of view it is not patentable; and, in fine, that it is not susceptible of any useful application.

"Being directly concerned as the inventor of the system of the distribution of electric energy, which is the subject of this article, I think I may claim from your courtesy the insertion of the following rectification.

"In reply to the first point, I must remind M. Marcel Deprez that Prof. Colombo's communication contains literally the following sentence:—'These apparatus are composed in principle of two helices, formed very ingeniously of discs of copper superimposed on and insulated from each other, the discs or spirals of one helix comprising between them those of the other.'

"Now I do not believe that the earlier researches of M. Marcel Deprez, Sir C. Bright, and M. Jablochkoff, on the phenomena of induction effected by means of Ruhmkorff's coils, can be confounded with those undertaken by means of the instrument above described, and which differs from Ruhmkorff's coil, not merely in its form, but in the fundamental principle of its construction, which I have claimed a long time ago, and which consists in the absolute necessity of the equality of the inducing and induced masses, and the symmetry of their position with respect to the magnetic field, in order to obtain the maximum yield in the recovery of the transformed energy.

"Therefore, until M. Marcel Deprez shall have demonstrated that an apparatus similar in form and principle has been constructed by himself or by others, I shall consider myself as the inventor of a new arrangement, both from a scientific and an industrial point of view.

"As regards the novelty of the results, the controversies carried on to the present day on the yield of secondary generators prove that our predecessors have never even conceived the possibility of obtaining them.

"Measurements, which I consider as exact, have been taken at the Turin Exhibition by the members of the international jury by a method, the defence of which I leave to MM. Joubert and Mascart, since an instrument invented by the latter of these *savants* was used in these determinations.

"These measurements establish that the apparatus which we submitted to the examination of the jury furnish, when employed to produce the work for which they were constructed, a yield of 89 to 90 per cent. The apparatus measured by MM. Joubert and Pottier was represented by us as furnishing an electric work of 2-horse power, or 1,492 watts, under the influence of a primary current of 12 ampères. This apparatus, composed of 1,000 copper discs, of 12 centimetres external diameter, with a space of 5 centimetres in the centre, and of ¼ millimetre in thickness, forms a column of 15 centimetres in diameter, and 55 centimetres in height.

"The weight of the copper employed is 20 kilos., and the price, consequently, is 60 francs. M. Marcel Deprez has, therefore, misinterpreted the communication of Prof. Colombo when he affirms that the mere price of the copper used in the construction of our apparatus amounts to 17 francs per Edison lamp, since with 60 francs worth of copper we construct an apparatus capable of supplying 18 to 20 16-candle lamps. Thus, though M. Marcel Deprez pretends that it will be easy to find varied arrangements answering the same end, and which are certainly public property, I still doubt whether, in spite of his inventive genius, he will be able for a long time to find a more economic arrangement.

"There only remains the question whether our apparatus are capable of rendering industrial service. But all along, or at least since the learned researches of M. Marcel Deprez and his predecessors have given a powerful impulse to the utilisation of the phenomena which electricity is capable of producing, the great concern of electricians has been to find industrial means of distributing electric energy. Without criticising here researches previously undertaken in this direction, we can say that no practical result has been secured in competition with that given by our secondary generator. I do not require to tell M. Marcel Deprez that one of the essential conditions for transmitting energy to a great distance is to give to the work transmitted the form of small quantity and high tension. But as, on the other hand, we do not know of any other instrument capable of utilising industrially this form of energy, save the machine which he has promised to construct, we thought that it would be simpler, more logical, and consequently more useful, to search out the conditions of the industrial establishment of apparatus capable of transforming energy transmitted to a great distance, so as to render it utilisable by all apparatus, lamps, and machines which modern industry has at its disposal.

"M. Marcel Deprez urges as an objection against us the necessity of employing for the working of our system for the distribution of energy, alternating current, which he pronounces dangerous. Not only is it possible for us to use currents of a uniform direction if they are interrupted or undulated, but we have good reasons for giving the preference to alternating currents. The first is that the principal circuit supplying all the secondary generators is closed metallically at the limits of the generator, whence it results that the points of the circuit which might be touched by consumers cannot have, at most, a difference of more than 100 volts, which is far from being formidable. On the other hand I do not believe that the system of the transmission of energy by means of continuous currents, such as M. Marcel Deprez proposes to establish offers a greater security. But the principal reason which leads us to adopt the alternative form for our primary currents is the simplicity with which great work may be obtained by means of known types of machines with alternating currents, whilst it seems to me very difficult, in default of proof to the contrary, to place upon dynamo machines with continuous currents, collectors capable of gathering great electromotive forces. Lastly, as to the independence of the apparatus it is absolute on our system. It is incorrect to say that the working of the lamps fed by our generators is compromised by the extinction of a single lamp, or of several, for the mobility of the magnetic field of our apparatus enables us, on the contrary, to vary at will the intensity of the current developed in their secondary circuit.

"As for the reproach urged against us that we can distribute merely light, if it were merited, still the fact of being able to utilise natural forces at distance for lighting up towns might satisfy the most impatient. But the difficulty signalised of producing an economic motor, by means of alternating currents, does not alarm us. If, for the production of motive power and for electro-chemical work, it is useful to have continuous currents, we shall effect on the secondary generators the same rectification of the current, as it has to be done with ordinary dynamos. It will be easy for us to supply to all kinds of machines the form of energy most suitable for their action, so that we have solved by the simplest method, not merely the question of the transmission of power to a distance, but the much more important problem of its distribution. But for the present we will simply claim the facts established, and for which our priority cannot be called in question.

"The experiments at Lanzo have taken place, not merely in presence of the jury, but have been repeated with complete success before the Italian Minister of Public Works and more than 300 Italian engineers and architects assembled at Turin by occasion of the fifth Congress.

"For five consecutive hours, the stations of Turin, Venario and Lanzo, connected with the gallery of the Exhibition by means of a circuit of 80 kilometers of chrome bronze wire, 3·7 millimetres in diameter, having a conductivity of 98 per cent., and courteously placed at our disposal by the firm Mouchel, have been lighted up as follows:

"The station of Turin with 34 16-candle Edison lamps, 48 8-candle lamps and one Siemens lamp; the station of Venario by two Siemens arc lamps; the station of Lanzo with nine Bernstein lamps, 16 Swan lamps of 100 volts, one sun-light and two Siemens arc lights. At the Exhibition there were burning nine Bernstein lamps, nine Swan lamps and one sun lamp, and in the Figaro kiosk, served by a small type secondary generator, five Swan lamps.

"In face of these results the jury awarded to MM. Gaulard and Gibbs the grand prize of 10,000 francs, given by the Italian Government. In spite of our invitation of January last, M. Marcel Deprez had the kindness not to come and contest it with us. Further, the fifth section of the Congress of Italian Engineers and Architects closed its works with the following order of the day:

"'The fifth section of the fifth Congress of Italian Engineers and Architects assembled at Turin in 1884, cannot overlook the great importance of the experiments on the transmission of electric energy to great distances, conducted at Turin by means of the secondary generators of Gaulard and Gibbs, and having been able to appreciate the working of this system of distribution expresses the wish:

"'That government, municipal bodies and manufacturers may support the establishment of this system, and that the hopes may be realised which six months' experience on the metropolitan railway of London, the experiments at Turin-Lanzo, and the sound conceptions on which this system is based, have excited in the scientific and industrial world.'

"The sole conclusion to be drawn from these facts is that the distance from Paris to Turin is too great to permit M. Marcel Deprez to judge the case with a full knowledge of the circumstances. But the illumination of the stations on the railway Torino-Ciriè-Lanzo will be continued until the close of the Exhibition, and the quarantine being now suppressed, the difficulty in the way of a full examination by those interested is considerably diminished.

"However this may be, it is with a legitimate satisfaction that in the midst of the wonderful signs of the development of Italian industry, of which Turin is at present the theatre, I have kept the flag of France at the head of the progress realised by the applications of science to industry."

THE ELECTRIC LIGHT ON THE INDIAN TROOPSHIPS.

IT is stated that the six hours' official trials which took place on Monday and Tuesday last week on board H.M.S. *Euphrates* mark the completion of the contract which was given to the Edison and Swan United Electric Light Company, Limited, for the lighting of the whole of the Indian Government troopers by the Edison system.

Commencing in May of last year, the company has, says the Portsmouth *Evening News* of the 30th ultimo, during the usual period which elapses between the termination of one trooping season and the commencement of the next, fitted the *Malabar*, *Crocodile*, *Serapis*, *Jumna*, and now the *Euphrates*, in the order enumerated, with Edison incandescent electric lamps from bow to stern, and almost from truck to keelson. The following table will doubtless prove interesting :—

Name of Ship.	No. of lamps.*		Dynamos.	Engines.	Speed.
	Full.	Half.			
Malabar	115	264	2 Edison	2 Brotherhood (3 cylinder)	525
Crocodile ...	98	288	2 Edison-Hopkinson	Do.	450
Serapis	101	300	Do.	2 Gwynne Invincible	450
Jumna	97	300	2 Edison improved	2 Brotherhood	280
Euphrates ...	98	301	Do.	Do.	280

Note.—Full lamps are of 16 candle-power; half lamps are of eight candle-power on the *Malabar*, and 10 candle-power on the remainder.

In addition to the above there are on each ship two large reflectors, each containing eight 50-candle lamps, which are intended to be slung to the yard-arms for the purpose of facilitating the operations of coaling ship, embarkation and debarkation of troops, &c., and are also found to furnish a charming illumination for the impromptu concerts and dances with which the ladies and officers relieve the monotony of their passage to India or home. The *Crocodile* is in addition fitted with mast-head and port and starboard lights of 50 candles.

No better instance could be quoted of the vast strides towards perfection which electric ship lighting has taken during the past 18 months than these admirable installations. Starting with the *Malabar*, the electric machinery of which runs at 525 revolutions per minute, whilst each dynamo is capable of sustaining but 150 full lamps, the speed has progressively been reduced to 450 on the *Crocodile* and *Serapis*, and 280 on the *Jumna* and *Euphrates*, whilst one dynamo is now capable of sustaining the whole of the lamps in each of the four latter ships, thus allowing the other machine to be held in readiness in case of any accident; indeed, on the *Jumna* and *Euphrates*, one machine is found amply sufficient to keep the two yard-arm reflectors going, in addition to the total lighting of the ship.

Again, owing to the lesser electromotive force required on the eight-candle lamps of the *Malabar*, it is there impossible to substitute full for half lamps, or *vice versâ*, whilst on the remainder all lamps take equal electromotive force, and it is therefore practicable to change the amount of light at any particular spot to suit the convenience or necessity of the occasion. The great advantage of being able to more than treble the lighting of the engine-room, for instance, in the event of a break-down occurring, is a boon which cannot be over estimated.

Another improvement over the first installation has been effected in the remainder by placing the lights in each cabin instead of in the pilastres, and we understand that it is contemplated to make this alteration, together with the substitution of half lamps of equal electromotive force to the full ones, on the *Malabar* at the termination of the present season. The saloons, engine and boiler rooms, are lighted with full lamps on all ships, whilst each cabin and mess table has a half lamp allotted to it. In each cabin there is a small switch, controlling its lamp, so that the occupants can obtain light at any moment during the night, or extinguish it at pleasure, whilst the mess table lights are controlled in groups, thus enabling the "lights out" rules of the Service to be complied with, a certain number of lamps remaining alight throughout the night as police lamps.

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* [The figures in the above table, it will be noted, are in one or two instances different from those given in the table reprinted from the *Times* a week or so ago.—EDS. ELEC. REV.]

Considerable improvements have been likewise effected in the wood casing, which covers the wires, which now appear able to stand not only the most severe deck washing, so dear to the heart of a blue-jacket, but also the intense heat of the stokehole.

A noticeable feature is the complete safeguard against fire by damage to the electric wires, which is attained by the insertion of a short piece of lead wire in the circuit of each individual lamp and main group of lamps, which is calculated to fuse long before, through any cause, a dangerous elevation of temperature can be set up in the wires.

In the direction of cost the superiority of electric light has proved most marked; for with a vastly increased amount of light the cost of the purser's dip has been beaten by 50 per cent., actual trial having proved that where considerably over £200 was expended on oil and candles, the electric light has cost but little over £100 to maintain, the item of renewal of lamps through failure of the carbon loops having proved exceptionally small; an average life of 4,000 to 5,000 hours actual burning having been attained through the care in manufacture taken by the company, and the rigid tests for the detection of faulty lamps which they make before issue from their factories, in which the immense number of 6,000 to 8,000 per week are produced. The whole of the work has been carried out to the complete satisfaction of the Dockyard officials — who have closely scrutinised it during its progress, and severely tested each ship as completed—by Mr. Douglas C. Bate, A.S.T.E. & E., assistant engineer of the contracting company.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.—MONTREAL, 1884.

ON THE SEAT OF THE ELECTROMOTIVE FORCE IN THE VOLTAIC CELL.

By Prof. Oliver J. Lodge.

(*Specially reported for the "Electrical Review."*)

Read before the British Association at Montreal, in Section A, August 29th, 1884.

[Preliminary Notes.—A copy of these Notes, which were placed in the hands of each member present at the commencement of the reading, has already appeared in our columns, see page 261, Electrical Review, October 4th.]

As this is the first formal discussion ever held in Section A, I may be permitted, in starting, to say with what hope I look forward to these discussions in the future, and how anxious I am that they should succeed. I have attended the meetings of the British Association consecutively for 12 years, and I have been more and more impressed with the small result that the sectional meetings have as compared with what might be expected, considering the magnitude of the men who frequently take part in them. In this section room physicists from all parts of the British Isles, as well as from Europe and America, are more frequently to be met with than at the meetings of the Royal Society itself, and the whole atmosphere ought to be favourable to a free and informal interchange of opinions most beneficial, instructive, and stimulating to the younger men, like myself, and to a great extent this is the case, especially when our present president is at the meetings, whether in the chair or out of it. But still, as a rule, there is a long list of papers to be gone through every day, the discussion on each being nipped in the bud in order to get on to the next and clear off the list. In favour of this practice of encouraging papers rather than discussions, it may be truly urged that useful discussions on abstruse papers, such as we are likely to have in this section, are almost impossible. Only in case one has been recently working at a similar subject does the ordinary physicist feel competent even to ask a pertinent question. Now it seems to me that abstruse papers, or papers on out of the way, and to most persons uninteresting, topics, rather should be communicated to one of the many learned societies whose business is with papers and their publication; and that communications to this meeting ought primarily to be on discussable topics, whether these be rigorously new or not; and if papers on fundamental and well known topics, concerning which there was still outstanding differences of opinion, if such papers were encouraged and definitely asked for in good time beforehand, workers might be encouraged to look up that particular subject specially, to read its literature, may be to make experiments in it, and generally to give it that careful thought without which discussions can neither be lively nor fruitful. It was with these ideas that the secretaries of the section last year agitated for the inauguration of such discussions, and, thanks to the energy of Dr. MacAlister, they now begin at this unique and most interesting meeting.

The subject chosen for the present discussion illustrates in a remarkable way the need for such conversation. It is scarcely credible, at the present rate of progress, that 84 years after the discovery of the voltaic pile opinions should still be utterly divided as to the seat of the E.M.F. in it. I venture to hope that it may now be decided, and a substantial agreement arrived at with respect to it. My business is to open the discussion, but it so happens that for some seven or eight years I have believed myself to see more or less clearly to the root of this particular matter, and a laborious review of the literature of the subject has only strengthened my conviction. Having, therefore, strong and definite views, I can hardly help letting them appear, and without assuming that these views are agreed with primarily, or even at all, they may yet serve as a link with which to connect the facts and the multifarious observations thereon.

In the course of my reading on the subject I have found only two great epoch-making papers—that of Volta in 1801, and that of Sir William Thomson in 1851. The other contributions are some of them keen, like those of Faraday, and of Pellat. Some of them are laborious, like Hankel's and Ayrton and Perry's; but none contain anything specially and powerfully new, except those two: unless, indeed, we include in the subject the immensely important phenomena of Seebeck and of Peltier, and Faraday's fundamental law of electro chemical decomposition. Volta showed that when two metals were put into contact and separated, the insulated one was charged with electricity sufficient to make gold leaves diverge. He also stated that the contact force between any two metals was independent of intermediate metals, so that the metals could be arranged in a definite numerical series; and he gave the first series of the kind—

$$\underbrace{\text{Zu} - \text{Th}}_{5} \quad \underbrace{- \text{SM}}_{1} \quad \underbrace{- \text{Fc}}_{1} \quad \underbrace{- \text{Cu}}_{2} \quad \underbrace{- \text{AG}}_{3}$$

Moreover, he started a hypothesis to account for the action, a sort of impulsion or attraction of electricity by matter, an idea subsequently elaborated by Helmholtz. Fabroni objected to Volta's explanation of his experiment. He denied contact force, and considered that electricity was developed by chemical action. Then the fight began, and lasted on and off some half century. On the one side were Volta, Davy, Pfaff, Peclet, Mariani, Buff, Fechner, Zamboni, Matteuci, Kohlrausch. On the other were Fabroni, Wollaston, Parrot, Oersted, Ritchie, Pouillet, Schoenbein, Becquerel, De la Rive, and Faraday.

It was not all fighting. Part of it resulted in a more thorough investigation of the voltaic phenomena, and very often the original point of dispute was lost sight of, and Volta's fact itself was doubted in the eagerness to disprove Volta's explanation. Pfaff's and Peclet's, and other experiments fairly well established the correctness of his observations, and Kohlrausch invented a condenser method of measuring the voltaic force absolutely, which has been employed with modifications by Hankel, by Gerland, by Clifton, by Ayrton and Perry, by Von Zahn and by most other experimenters on the subject. Wherever electrostatic methods were employed, and when the electroscope was the instrument of research, the contact theorists had it all their own way, and it was only by apparent efforts and twisting of experiments that the chemical theorists could maintain their ground, but when electric currents were dealt with and the galvanometer used, then the chemists had their turn, and they showed most conclusively that no mere contact could maintain a current unless heat disappeared at the junction, a fact obvious enough to us to-day, on the principle so laboriously and finally established by Joule. By means of the galvanometer the contact theory was so belaboured by Faraday, that it ultimately seemed to give up the ghost, and the chemical doctrine triumphed; so much so, that Volta's original fact, in spite of the evidence which had been accumulated, was again doubted, and one finds in the text books culled from this period statements that Volta must have rubbed the plates together, or that there was moisture in the air. Also hints are given that films existed on the plates, that coats of

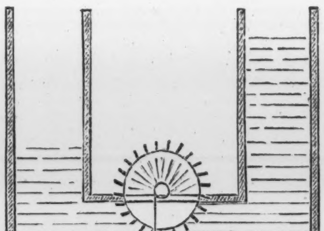

Fig. 1.

Prof. Lodge's hydrostatic analogue of the real difference of potential produced by a Seebeck force; and of the Peltier effect. W is a weight driving a water-tight wheel until stopped by the difference of level set up.

varnish or lacquer might produce electricity, and so on. It was pointed out by De la Rive how minute a trace of chemical action could produce how much electricity, and how little electricity could affect an electroscope. But it is to be noticed that any chemical action caused by damp on the plates or moisture in the air would be of the nature of local action, and local action is not a satisfactory producer of manageable electricity.

Quite detached from any connection with the controversy, because at that time quite unintelligible to all but one or two here and there, two papers appeared in 1851 by the president of this Association, which were the triumph and apotheosis of the chemical theory of the source of the current in the voltaic cell. In one of these papers, that on *electrolysis*, it is irrefutably established on the basis of the conservation of energy, that making exception of such irreversible effects as are not readily brought into calculation, and allowing for certain possible reversible facts to be investigated thermo-electrically, the E.M.F. of a cell is not only dependent on the chemical action going on, but is calculable chemically in absolute measure from purely chemical data supplied provisionally by Dr. Andrews. It is proper to say, however, that this brilliant theory is avowedly based on the laborious and acute experimental work of Joule on the conservation of energy in the voltaic circuit. In the other of the two papers, that on thermo-electricity, it was shown that from the fact that the current absorbed or generated heat at the metallic junction, an E.M.F. was necessarily existent there; in other words, that the Peltier effect necessitated the previously discovered Seebeck one. The establishment of the conservation of energy by Joule for ever placed beyond doubt the fact that the energy of the electric current produced by a battery was due to, and was the equivalent of, the chemical actions going on there; but it was supposed, and is still supposed, though, as I venture to think, quite erroneously, to leave untouched the question as to the precise seat of the E.M.F. in a battery. However that may be, the success of the chemical theory of the electric current naturally causes it to be still more certainly assumed that the apparent contact force of Volta could also be accounted for by accidental chemical action, and that without some chemical action somewhere no Volta effect could be produced. This also, I believe, to be quite false.

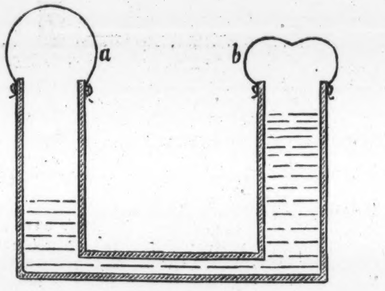

FIG. 2.

Prof. Lodge's hydrostatic analogue of the *apparent* difference of potential produced by contact, or of the Volta effect. *a* and *b* are air-tight elastic bags differently stretched.

The triumph of the chemical theorists with regard to the Volta effect was, however, short lived, for in 1860 to 1870 the invention of the quadrant electrometer put into the hands of electrostatic experimenters a far more refined and delicate instrument than could have been thought possible a few years before, and the illustrious inventor of that instrument himself for ever put the truth of Volta's phenomenon beyond doubt by the most simple and beautiful device of suspending a charged torsion arm over a zinc-copper junction. By comparing the deflection so produced with that caused by a Daniell cell an absolute measure of the so-called contact force was made, and it was shown that uniting the copper and zinc by a drop of water instead of by a metal, no deflection was produced. It was also shown that the deflection was greater with clean zinc and oxidised copper. But Sir William Thomson went further than this. He sounded a theoretical note, and in a sentence revived the whole controversy about the seat of power in the pile. The sentence is this:—"For nearly two years I have felt quite sure that the proper explanation of voltaic action in the common voltaic arrangement is something very near Volta's, which fell into discredit because Volta, or his followers, neglected the principle of the conservation of force. I now think it quite certain that two metals dipped into one electrolytic liquid will (when polarisation is done away with) be at the same potential." And then he goes on to one of those brilliant and extraordinary speculations characteristic of no one else, and applies this apparent contact force to determine an upper limit for the size of atoms, an application obviously of transcendent interest, and of more importance than all the previous outcome of contact discussions put together.

The whole subject now acquired a fresh interest, and a new series of experimental determinations of contact force began. Hankel's and Gerland's measurements belong to this period in point of date, though in method and motive they probably are the outcome of the earlier period. Hankel uses a modified Kohlrausch method, and Bohnenberger's electroscope. The especial merit is the determination of contact force without introducing blotting paper, glass, or fingers, as the earlier experimenters had done. Clifton employed Kohlrausch's method in 1877. He used a Thomson electrometer and a Clark's cell as standard. He has only published a preliminary paper, in which he overlooks minutiæ such as the change of the contact force by adventitious circumstances. When he touches on theory, he agrees with Thomson. Ayrton and Perry, in 1876, devised in Japan a very ingenious but somewhat cumbersome and unwieldy modification of Kohlrausch's method, and with the help of students carried out a most extensive series of determinations of metal-metal and metal-liquid and liquid-liquid contacts. On the appearance of Clifton's paper a year after on the same subject, they issued a strongly-worded claim in respect both of priority and completeness, a claim which seems to me well established, since their results are the most comprehensive yet obtained; and the energy needed to devise, construct, and use such an apparatus as the one they depict must have been immense. A convenient summary of their work is to be found in Everett's Units and in Gordon's Electricity, both second edition. The main result achieved by them was the experimental establishment of the summation law for any substances (this is not to be confused with Volta's summation of series law, which is only applicable to metals), viz., that the total E.M.F. of a closed circuit of any number of substances may be reckoned by adding up the Volta forces observed electrostatically for every pair of substances in contact. This law is, it seems to me, in the highest degree probable theoretically, but still it is important to have it experimentally established, especially as they point out that it is often called in question without good ground. The establishment of this law is perhaps their main work in this matter. Clifton arrives at the same conclusion, and gives a handy diagram of the contact forces at different junctions. My own opinion is that the intended and obvious significance of these diagrams is theoretically wrong; but they embody certain experimental results conveniently, and can be interpreted properly. I believe Prof. Clifton, as well as Profs. Ayrton and Perry, have made many more experiments besides those recorded in their communications to the Royal Society; but as they have not been published I can give no account of them.

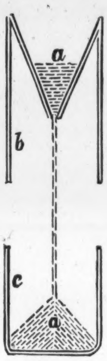

FIG. 3.

Sir William Thomson's Volta-electric electrophorus or gravitation voltaic cell, in which gravitation plays the part that chemical action does in the ordinary cell. *a*, copper filings; *b*, inductor-zinc; *c*, receiver.

Among the Thèses presented to the faculty of sciences in Paris in 1881, we find an important memoir by Pellat, which reviews the whole position very clearly, and records a series of determinations of Volta forces among metals which are evidently the most accurate and satisfactory yet made. He adopts the capital plan of neutralising the charge of a condenser by the Poggendorff method, and thus converts Kohlrausch's into a null method for which a very sensitive electroscope is all that is needed. The plates of the two metals set face to face are connected, not directly, but by a greater or less length of a graduated wire conveying a current, and the position of the slider on the wire is adjusted by continually separating and testing until no charge at all is found. The step of potential on the wire is then precisely equal to the contact force between the plates which would have caused the charge in a similar uncompensated condenser. Compensation methods of a sort had been used before by Girland and by Thomson. Girland applied compensation to determine the E.M.F. of two liquids dipped into a liquid, and Thomson applied it to the divided ring experiment, thus making it very analogous to that of Pellat. Pellat also takes Sir W. Thomson's view that the effect is due to a true contact force between the metals, and that it represents a real difference of potential between them. At the same time, he is careful to point out that no rigorous proof can be given of this, and that all that is actually measured electrostatically

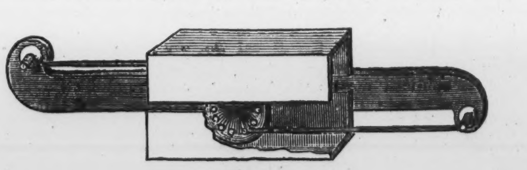

FIG. 4.

Sir William Thomson's voltaic replenisher or inductive electrical machine excite by contact force.

is the difference of potential between what he calls the electric coats or what may be more simply called the air films on the two touching surfaces.

Meanwhile, some experimenters, starting with a belief in the chemical origin of the Volta effect, had made experiments supposed to support this view. Mr. J. Brown, of Belfast, in 1878, repeated Thomson's divided-ring experiment, as well as Kohlrausch's condenser experiment, in other gases than air, and found a very decided difference, and even a reversal of sign when sulphuretted hydrogen was substituted for air. The metals he used were copper and iron, and he obtained a 1-centimetre deflection in a direction indicating iron + in air, while in Sh^2 he obtained a 3-centimetre deflection indicating that iron was −. On readmitting air the deflection again reversed; and so on, until the copper coated itself with a blue film of sulphide, when the deflection became undecided, owing as he supposed to the cessation of chemical action. In 1881, he observed the volta effect with a copper-zinc junction and reckoned that at the first instant after cleaning the potential difference would be a ·9 Daniell, which he says agrees with J. Thomsen's determination of the difference of the heats of the combustion of zinc and copper and oxygen. He here gives a hint holding the heterodox notion which I do not find in any other of his writings and which I believe he has abandoned, even if he ever really held it. In 1879, Brown tried a copper-nickel divided ring, substituting H.C.L. for air, and then he also succeeded in obtaining a reversal of sign. He also arranged a divided ring of wet blotting paper and showed that there was a difference of potential when the two halves were touched with a zinc-copper couple (which is not remarkable), but he then goes on to draw a moral, and to say that the slit of the divided ring corresponds to the air film and the wet paper to the moisture film in an ordinary volta condenser experiment. The film of moisture on the zinc plate is thus shown to have a plus charge and that on the copper a negative. If it be objected that the better the plates fit the better the manifestation of contact E., it is to be replied that it is not to be supposed that there is *no* air between them anyhow, says Brown. Probably, he says, you produce the difference of potential only so far as it forms a film on the surface. When a metal and a liquid are experimented on it is probably a two fluid cell, the other fluid being that condensed on the surface of the metal. Brown thus goes hot and strong for the activity of the films of condensed air which are certainly on the surface of solids and which may play an important part in the matter. But he supposes that these films act by corroding or attacking the plates, and that such a film is necessarily existent between surfaces nominally in contact if any volta effect is to be produced, so that if the metals really and truly touched all over, they would show no charge when separated. Moreover he lays it down that the potential difference is only observed while chemical action is going on, but that so soon as it ceases from any cause at once the volta effect ceases. In all this I entirely differ from him: but his experiments are very interesting, and much to the point.

They cannot however be regarded as settling the question, the very important and fundamental question, as to whether the volta effect depends on the atmospheric or medium surrounding the plates, or whether it is an absolute effect dependent on contact. Experiments on this point are absolutely discordant, and it seems to be one of these points which it is very difficult to settle by direct experiment. For if by using a chemically active gas instead of air you get a positive result or change in the volta effect, the answer from the other side is: "Yes, of course; because your plates are corroded and coated with sulphide or chloride or something whose contact force comes in and modifies everything." If on the other hand you get a negative result when you substitute some inert gas like hydrogen for air, then it is objected that you have not removed the air-film which the plates had contracted from long standing in the air, and if you answer indignantly that you did, and that your hydrogen was perfectly pure, it is replied with a sneer:—"Oh, yes. It is not so easy to get pure hydrogen as you seem to think." Moreover, suppose a positive effect in changing the gas was established, what then? Nothing is settled, except the metal air contact force is somewhat different from the metal-gas contact force. There seems to be really no way of knocking contact force on the head experimentally, and this, probably, because it is a reality. *There really is contact force at every junction of dissimilar substances, and the E.M.F. of a circuit, whether it be inductive or conductive, is always the sum of such contact forces.* I do not say that the contact force at any given locality has the value ordinarily assigned to it as the result of experiment. The earliest attempt to examine the question as to whether the Volta effect depended on the atmosphere was made by Pfaff in 1829, who used dry and damp air, oxygen, nitrogen, hydrogen, carburetted hydrogen and carbolic acid, and he found that there was no difference so long as no visible chemical action occurred. De la Rive, on the other hand, asserted that there was no Volta effect in the slightly rarefied air then known as vacuum. In recent times Pellat has investigated the subject, and has come to a conclusion in agreement with Pfaff, namely, that the difference is very small. The gases he used were dry and damp air, dry oxygen, dry nitrogen, dry and pure hydrogen, and dry carbolic acid at different pressures. Copper and zinc were the metals employed.

Von Zahn also tried platinum and zinc condenser in the highly rarefied air now known as vacuum, with some melted sodium in a drainage tube to absorb all the oxygen, the Volta effect was diminished, and only represented a potential difference of half a Daniell. I am not clear whether sodium can be trusted to ultimately absorb every trace of oxygen, but I should judge it would take a very long time; and as to rarefaction—dividing the numbers of molecules in a vessel by a million or two, leaves them quite numerous enough to accomplish anything they want. The views of Ayrton and Perry on the subject of the effect of the atmosphere underwent modification between their first paper and their third. In the first paper they say "they have good reason to believe that there is no great difference of potential between a metallic or liquid surface and the air in contact with it." Clerk-Maxwell, in a letter to the *Electrician* in 1879, poked fun at them for this, saying, "A statement like this coming from men whose scientific energy seems likely to displace the centre of electrical development, and to carry it quite out of Europe and America to a point much nearer Japan, is worthy of all attention, even without an explicit statement of their 'good reason;' but Mr. J. Brown has shown (*Phil. Mag.*, August, 1878) by the divided ring method of Sir W. Thomson, that whereas copper is negative with respect to iron in air, it is positive with respect to iron in hydrogen-sulphide. It would appear, therefore, that the reason why the results of the comparison of metal by the ordinary 'contact force' experiments harmonise so well with the comparison by dipping both metals in water or an oxidising electrolyte is not because the electromotive force between a metal and a gas or an electrolyte is small, but because the properties of air agree, to a certain extent, with those of oxidising electrolytes. For, if the active component of the electrolyte is sulphur, the results are quite different, and the same kind of difference occurs when hydrogen-sulphide is substituted for air."

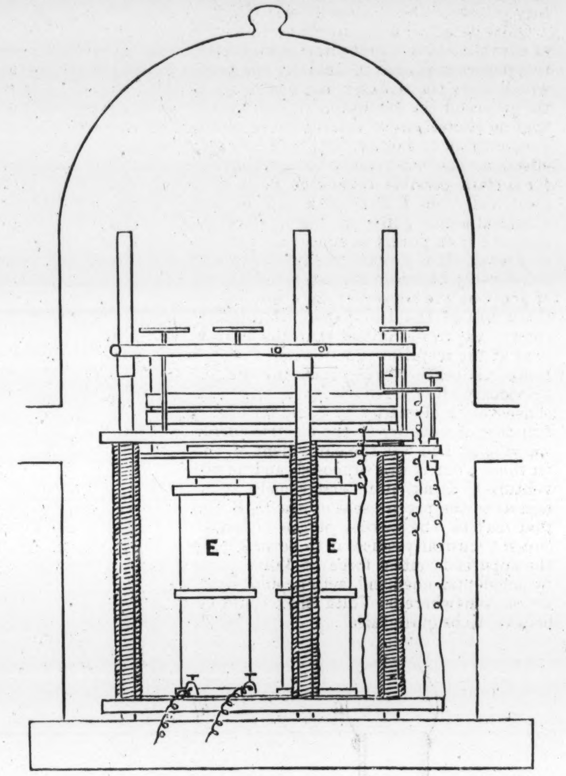

FIG. 5.

Pellat's apparatus for measuring the Volta effect in different gases at different pressures by means of a compensation method. The electro-magnet, E, is used to separate the plates the required small distance.

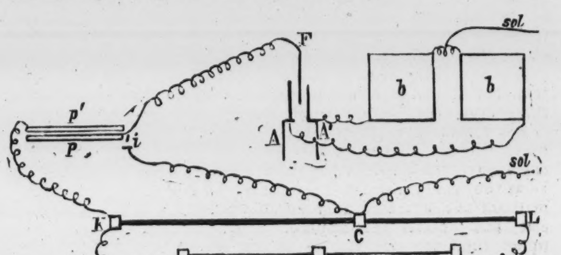

FIG. 6.

Diagram of connections in Pellat's compensation method of measuring contact force. K L, compensator; C, slide; R, rheostat; P, Daniell's cells, furnishing the current to the compensator; *p p'*, condenser plates, the lower fixed, the upper movable; *i*, interrupter; F, gold leaf; A, A', electroscope plates; *b*, *b*, batteries of 100 volts each for charging the plates, A, A'.

In their third paper, therefore, we find their views changed, and they clearly point out that their experiments, like all inductive methods, leave the air contacts quite undecided. They then go on to say :—"One way of determining the E.M.F. of contact in volts between a substance and air, and one which we hope shortly to employ, is to repeat all those exact experiments in different gases. (It is not quite true that this would give the result aimed at, because it would give differential effects, very important to observe, no doubt, but not the same as observing the actual contact force of air or gas.) We shall thus ascertain whether the contact difference of potentials of a substance and a gas differs much for different gases. Qualitative experiments in this direction have already been made with very interesting results by Brown of Belfast, but his experiments differ from ours in not being quantitative." Only, as their experiments do not yet exist, Mr. Brown's have still some value.

(To be continued.)

THE NATIONAL CONFERENCE OF ELECTRICIANS, PHILADELPHIA.

(Reported specially for the "Electrical Review.")

(Continued from page 351.)

HERE is another form of magnet (fig. 7) ; that machine would be defective. It has two magnets in the place of one, and it has two magnetic circuits in the place of one; and many of the lines of force probably make little private circuits of their own around in that way (arrows). Those lines of force are of course lost, and therefore it is more or less defective in that respect. There are machines which this figure represents exactly, but it is not meant for any particular machine; it is meant for a scientific drawing. I am only giving a general idea of the principle of these machines, and I do not refer to any particular machine.

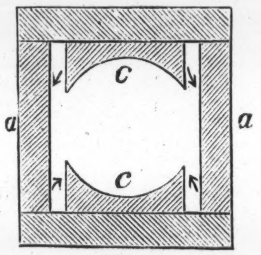

FIG. 7.

It is also important that this line of magnetic induction shall find easy passage around in order to produce the most intense field. Suppose this was the pole, *c*, then the opening at the sides must be made as wide as possible; everybody recognises that! The reason for that is in order that the lines of force may find easy passage across this opening, *c* to *c*.

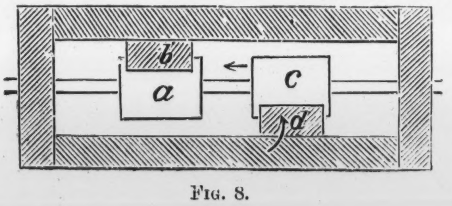

FIG. 8.

Suppose we had a machine made in the following manner (fig. 8). I do not want to say that it is anybody's machine; I may not have made the drawing right; but, suppose there was such a machine, and that somebody did make it, in which there was a magnet with a Gramme ring here, *a*, and a pole piece here, *b*, one ring here, *c*, and another pole piece here, *d*. How are the lines of force to pass? I do not know that it would be easy to see how; there is no easy passage. They evidently go around here (arrow) and strike into the Gramme ring, and I suppose pass across the opening here (arrow) and go to the other side the best way they can. You see there is no easy passage around for the lines of force in this case.

A MEMBER: May they not to some extent follow the shaft?

Prof. ROWLAND: It may be possible for them perhaps to go down and around, but it is evident that if the shaft is made large enough they come together around in that way, from *d* by shaft to *b*, but it is not so easy for them as to go across.

I have here a formula for the amount of work which one has to put upon a magnet to produce a certain effect. I will take the case which I have considered most efficient, where there is one magnetic circuit. Here you will have the lines of force going around this machine in this manner.* The reason they go around is from what I have sometimes called a magneto-motor force. It is not a very bad term to be used. It is an original idea of Faraday that these lines of force are conducted by a metal. We suppose these lines of force pass around this (arrows), and then they have to pass through the iron, and across the opening in this way (indicating), and they are caused to do that by what may be called the magneto-motor force of the helix. The magneto-motor force of that helix is proportional to the number of cores around the magnet, and proportional to the current, C, in the wire. I won't write down the exact expression for this quantity; I will only say the magneto-motor force is proportional to N, the number of turns around the magnet, and C, the current. The number of lines of force which pass around here we may call B. This is not the quantity which Maxwell considers, but it includes the whole number of lines of force which pass through the magnet. We may write B, proportional to N, the number of turns of the wire, and C, the current; and inversely proportional to the resistance of these lines of force in going around this circuit. The resistance of the lines of force is proportional to L. the length of the iron of the system, divided by S, the surface cross-section of the magnet, supposing it to be a uniform surface into π, the magnetic permeability of the iron (or you may call it the conductivity of the iron for the lines of force). This quantity, π, varies with the current; it can readily be obtained; indeed, some years ago, I gave a formula for it. It can be expressed simply as dependent upon the magnetism of the iron and a constant depending upon the iron alone. Then to this we have something more to add.

Let 1 be the width of this opening (indicating), and A, the area across which the lines of force flow; then we have to add $\frac{1}{A}$; we have still to add to this a certain quantity which we can call *p*, which depends upon the resistance of these lines of force which escape in all directions. It represents loss due to the escapement of the lines of force. Thus we have the final value for the magneto-motor force

$$\frac{N\,C}{\frac{L}{S\,\pi} + \frac{1}{A} + p}$$

This gives us an equation which may be solved without respect to π.

It is evident that the curve for the magnetic permeability is of this nature. It rises to a maximum here, and then decreases. It will not rise as much as this diagram (fig. 9) indicates, but it will be of a more flat form, according to the value of 1, and *p*. Therefore, increasing the magnetic force on your magnet, a certain point will be arrived at where it becomes very easy to magnetise a magnet, and after that it becomes harder and harder to magnetise the magnet. So that in practice you should get the form

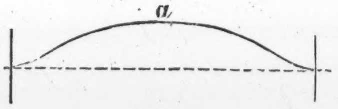

FIG. 9.

of this curve, so that it should produce a very strong magnetic field; this can only be done by an exact calculation, or better by experiments on the machines, examining at each point the force of the current, and in that way getting an idea of how these lines of force go, and we can then see whether this area is large enough to take all the lines of force necessary to pass around. The mathematical investigation is a long one, and I cannot take it up here. I have here an expression for the work necessary to keep up a proper field in the magnet. Of course, as you coil the wire on these magnets, the coil becomes thicker and thicker, and in order to have a sufficient magnetic force across this opening with sufficient velocity you must have sufficient wire. As you increase the thickness, of course you have to use more wire for a certain core. Every core acts very nearly the same as every other in producing force; therefore, if you have the magnets very short indeed, you have to pile up your wire to a very great extent. The wire is made of copper, and as iron is cheaper than copper it might be better to lengthen out the core slightly. I don't know where the lengthening should end, but I should suppose you would lengthen out the core until the given length of wire on the magnet may make a moderately thin layer. Of course, as you lengthen out your magnet, the resistance to magnetisation becomes greater; but that is a very small quantity; I do not suppose it increases very much for a considerable lengthening of the magnet.

As I said before, the magnetic conductivity of iron is many times greater than that of air, and we can lengthen out the cores without producing much loss on account of that lengthening. Some persons have suggested that there may be a slight gain from the fact that iron after it has been magnetised a great number of times in the same direction, rather likes to be magnetised in that same direction afterwards. If the core is made of any material similar to steel, such as wrought iron, or anything of that sort, it might be possible to have some gain from the coercive power of the magnet. There would be a loss from that cause;

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* See arrows in fig. 3, ELECTRICAL REVIEW for last week.

you have to act against the force of the magnet at first; but from the continual use of the machine, I think very likely the iron might get a set in the direction of the force which might possibly aid in the production of a suitable dynamo. If the core was of steel it is evident that you could send a strong current through at first and magnetise the steel, and then diminish the current slightly. We could thus still keep up a very large force at this point and with a moderately small current. I do not know how probable that would be, but it seems to me that one could produce a very strong field in that way. It may possibly be of practical value, and it might be tried. In the commencement of the operation of the machine we should have to send a powerful current to magnetise the steel, and then, without stopping the current, to diminish it; then the force of the steel would be in the same direction with the current and produce the field with less expenditure of energy than if it were simply iron.

As far as the difference between a shunt machine and a series machine is concerned, there is no difference. The magnetising force of this magnet is proportional to the number of turns multiplied by the current; that is, proportional to the cross-section of the coils multiplied by the current per unit of the section; so that the magnetising action can be the same with a machine which produces a strong current and a machine which produces a weak current. If you calculate the energy, you will find it is the same, because it depends upon the current into the cross-section. If the exterior dimensions of the coils are the same in both cases, the same energy is expended in each in order to produce the same force, therefore there is no difference between a shunt machine and a series machine; I think the energy will be the same for both.

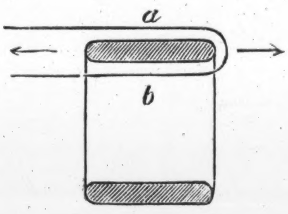

FIG. 10.

I do not wish to take up too much of your time, and I will go right on to the heating of the armature. Of course, the amount of energy expended in the heating of the armature will be dependent on its resistance; and the efficiency of the circuit will merely depend upon the relation between the resistance of the armature and the exterior resistance; that is very well known. There is one other point; there is a certain lot of dead wire, you may call it—I do not know the technical term, I think very likely that that is the technical term. In the first place I will only discuss the Gramme and Siemens armatures. In the Gramme machine you have the armature inside of the ring, and the ends do not act at all. In the Siemens armature, we have the coils brought over the ends of the armature. It is only a question of the work of the machines, and the relation between the two. In the Gramme machine it must be less than one-half of the wire that is acting. You have the section of the Gramme ring like that (fig. 10), and when that portion of the wire, *a*, is active, of course this portion, *b*, inside is dead. I suppose some of the lines of force go out here (arrows), so that we can see at least half the wire is dead wire.

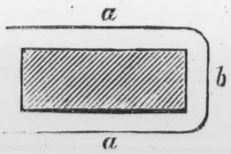

FIG. 11.

In the Siemens armature one cannot say immediately how much dead wire there will be, because it depends upon the length of the armature. In the Siemens armature you have the wire wound around in that way (fig. 11), and this portion, *a*, would be active and this portion, *b*, dead. Of course, if the armature is very thick, you would have more dead wire than when it is simply long. I cannot say which has the more dead wire, but I dare say the Gramme has more than the Siemens. Furthermore, either in the Gramme ring or the Siemens armature—I will draw this figure to represent either the Gramme or Siemens armature—you have the lines of force running across here (fig. 12), across here, dotted lines; that portion is active, and this portion, a', in between the poles is dead; and when the thing revolves you have the lines of force turning around, and I think that would add more dead wire to your machines. I believe an attempt has been made to throw out these coils; I believe in some form of machines they are thrown out; this is a practical point which everybody can understand, and if I should look over the Exhibition carefully I should find some machine in which this is taken account of. There is no necessity to go further. Of course, the efficiency of the circuit depends upon the ratio of the resistance of the armature to the resistance of the wires, and therefore, as far as this point is concerned, any machine can be just as efficient as you please by putting in greater and greater external resistance. But as the magnet remains the same you would find a point where the efficiency of the machine as a whole would not increase for an increase of external resistance, and the work would actually diminish. There are other things to be taken account of; there are some losses due to the self-induction of the coils which produce sparks in them. I have requested Prof. Fitzgerald to take up that point, and therefore I will leave it for him to consider.

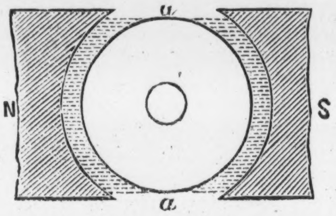

FIG. 12.

Now I shall consider another point with regard to the dynamo which can be treated in this simple manner with no use of the calculus. I think it can be done by very simple reasoning if you only know the principles. I shall now attempt to construct two machines similar in all respects, but one of which is larger than the other.

We can suppose that we construct one of the machines and then see what the effect will be when that machine gradually grows and becomes very large.

The point we shall start from is that the magnetic field is constant in the two machines. Owing to the fact that there is a limit to the magnetisation of a magnet, we cannot have a field which is produced by iron with more than certain strength; therefore, I suppose that you get up to a reasonable strength with your iron. It cannot be up to the maximum strength of course, but it is somewhat near it. I made some experiments many years ago upon an ordinary magnet, and the results were published, I think, in Siemens's journal; it is beautifully applied to the dynamo machines, and I obtained everything with it that I have referred to here. With that apparatus, if I remember rightly, I got about one third of the force in the field of a magnet that iron could possibly give; of course you could get greater. It is theoretically possible to get a force equal to the magnetisability of the iron, or the maximum magnetism of the iron; but, practically, I suppose the case is very similar to that of the ordinary dynamo machine.

Therefore we can say that the field of force in the two machines, one of which is larger than the other shall be constant, and I will therefore write down F = constant. It is not only constant in one part of the machine but in all parts; the magnetising force at every point of one machine is equal to that in a similar point in the other machine. Therefore if you drew the machines on the board it would not make any matter about the scale of dimensions of the machine; you could mark down the force at a certain point as being a certain amount, and consider the scale whatever you please.

Starting from that point, we will consider what must be the current in the wire in the two machines. The machines have the same number of turns around the magnet, and everything is the same, except that the dimensions of one machine are greater than the dimensions of the other. I will give you a formula for the current in the wire: suppose you have a tangent galvanometer, consider the current passing around its coil. If you make the circular wire of the galvanometer grow in order to produce the same effect at the centre, you must add current in direct proportion to the radius of the coil. If you make that coil twice as large you must have the current twice as large in order to produce the same force at the centre, and not only at the centre but at every point. If the material is constant there is no difference in the iron, we have the current in direct proportion to the linear dimensions of the figures, so that the current in the wire varies as the linear dimensions of the machine. Make one machine twice as large as the other and the current per unit of area in the magnet must be twice as great to produce the proper motive force; of course the wire has increased in size. If the machine has increased to twice its original size, the cross section of the wire has increased four times; in other words, from that cause the current per unit of area will vary inversely as the square of l, the linear dimension; and since we have found the current to vary directly as l, in order to retain the same force in the field by a combination of the two results, it varies inversely as l. Therefore as far as the magnets of a machine are concerned, if the machine grows larger in this way its heating effect, which depends upon the current per unit of cross section, will be less, since the surface will have been increased in proportion to the square of l; there will therefore be less danger of heating in a large magnet than in a small magnet; this is only with respect to the magnet. In some cases it is practically found harder to run a large machine than a smaller one, but we shall find how that comes.

The resistance of any part of the machine or the work of the armature varies, of course, directly as the length of the wire, and inversely as the cross section. The cross section varies as l squared, so that the whole work varies inversely as l, or the resistance varies inversely as l. Therefore the larger you make the machine the less the resistance will be; one machine being twice as large as the other, the resistance will be half as great.

I will now consider the electromotive force. This applies not only to the work of the magnet, but also to the work of the armature: we have here the current and the resistance. The electromotive force is proportional to the product of the current and the resistance, hence we may write $E = R C$. Therefore the electromotive force of this machine will be constant. We shall presently see how this is modified, so as to get the electromotive force greater for the larger machine. The work done is therefore equal to $C^2 R$ in any part of the machine, or in the whole machine, just as you please. But that quantity varies directly as l, the linear dimension. Therefore the one machine which is twice as large as the other requires twice as much power to run it, and you get twice as much electrical energy out of it. But it is to be remembered that the weight of the machine varies as l^3, and you have only got l times the work out of it as constructed in this way.

Let us see what the reason of this is. The field of the magnet is the same. There are just as many lines of force in the armature in the two machines, and when we calculate up we shall see that the angular velocity of the armature, or the number of turns per second or minute, must vary inversely as l^2. In other words, in one machine twice as great as another, the velocity of the armature must be only one-fourth as great, in order to produce the same current in the wires. This takes account, I think, of every irregularity in the machine; the two machines are exactly the same in every respect. I have not added the loss for the self-induction of the coil. I have an idea that this also should be taken into account, but Mr. Fitzgerald will consider that point.

So far as the results go, we have constructed two machines which differ only in size. The efficiency of these two machines is a constant quantity. That will be rather startling to some who come to me constantly and say "a larger machine is more efficient than a smaller one." As far as this thing goes, we have constructed a machine which is larger than another, and in which the efficiency is the same.

Let us see the cause of the efficiency remaining constant. On what does the efficiency depend? We have got to keep our magnetic field the same in both cases; and therefore the work done in sustaining the magnet will be this: ($C^2 R$). But here we have the angular velocity inversely as the square of the linear dimension. The question comes up again whether we cannot increase the velocity of the armature above that point. Is it practically necessary that we should run one machine at one-fourth of the velocity if it is twice as large? It is a practical question as to what we can get; I should think it certainly was not in this proportion, I should think it would be more nearly inversely as the size, and not inversely as the square of the size. Can we not then run the machine with greater angular velocity? We certainly can do so by increasing the external resistance, so that the same current flows when you run it fast, as in this case. Then the driving force will be the same, and the whole machine will be increased in efficiency. We can do that to a certain extent; I do not know where it will end practically; but starting with a small machine, without increasing the electromotive force which is constant in these machines, but by arranging the wire of the armature so as to increase the external resistance, we can increase this speed, and yet have the current per unit of section through the armature the same as in this case. If we do that, we shall be able to increase the external resistance of the machine in comparison with the resistance of the armature, and when we do that we increase the efficiency of the machine.

I think it is from this cause that we find large machines more efficient than smaller ones. But it is also evident that there is a limit which can only be obtained, I suppose, from practically making the machines, and seeing how fast they may be run without flying to pieces. If we can keep them going at a greater rate of speed than I have pointed out as they are increased in size, I suppose the efficiency will be increased.

As far as this theory goes, the increase comes not from the size of the machine but from the fact that you can get a greater electromotive force with the same angular velocity, and so can increase the external resistance in proportion, so that you find in very large machines you can make the wire with one turn around the armature, not several turns—just have bars on the machines. We thus decrease the resistance of the machine, and at the same time we run it above this relation which I have pointed out, and so obtain the proper electromotive force. The proper electromotive force is more easily obtained from the large than the small machine, because it is not practically necessary to decrease the velocity. I will ask Prof. Fitzgerald to continue the discussion on the loss due to self induction, and then the discussion will be open.

Prof. **FRANCIS J. FITZGERALD**: Mr. President and gentlemen, I feel that I have to apologise to you for having been led into the question of the effect of self-induction; and I have partly to apologise for not having had sufficient energy to investigate this thing very carefully, and that my information upon the subject is not brought as distinctly to mind as it might have been. The warmth of the weather and the incurable hospitality of the American people have not left me time to investigate the matter. The point which I was led to call attention to frequently some years ago, and which has been investigated and experimented upon by several, is this:—Taking a coil in which the current is being started from there to there (*a* to *b*, Fig. 13); the current is going in this direction (small arrow), and the ring is turned in that direction (large arrow); when the coil was at that side, *d*, the current was running down through it in that direction (arrow); as soon as the current passes across the brush at *b* it is reversed, the current passing off. Consequently the current which was at one time running in one direction, is reversed so as to cause the current to be running in the opposite direction. That in itself necessitates no loss of energy. The work that is to be done in stopping the current and turning it in an opposite direction does not in itself involve any loss of energy, if energy in stopping the current could be utilised in starting it again. If you could use the energy which is necessary to stop the current to start a fresh current in the opposite direction, there would not be necessarily any loss of energy in your arrangement; but what I want to call attention to is this; that in general a coil is short-circuited when it is passing from one of these positions to the other across the brushes, and consequently the energy in stopping is used in sending sudden and short currents through itself. There is in general a circuit through the coil, which is in short a short circuit in itself, and the energy of the coil which goes out in stopping is not used in starting the current in the opposite direction. Therefore you have lost the energy of the coil due to its own induction through a short circuit.

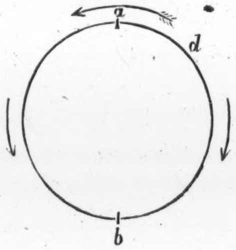

FIG. 13.

The loss of energy due to this cause for this coil will be proportional to the induction and the square of the current passing through it. If M is the induction of each coil, and C the current passing through it, $M\ C_2$ is the loss of energy due to the self-induction of the coil being wasted on the local current. This will be on each coil; hence, if N were the number of coils that were passed per second, $M\ N\ C_2$ would be the loss per second owing to this fact. This can be put into another form by substituting for $M\ N$ the terms of the self-induction of the unit of length of the ring. If V is the velocity of the edge of the ring, and L is the induction per unit of length on the ring, then necessarily $V\ L = M\ N$; and $V\ L\ C_2$ will represent the loss of power owing to the self-induction of the coils. For a given ring it varies directly as the linear velocity of the ring. It was a very difficult thing to make experiment upon this effect as to whether there was any loss of power due to the self-induction of the ring. The term $V\ L$ is in the nature of a small resistance, which we may call R, so that the loss may be written $R\ C_2$. This is a resistance of the machine due to its velocity, and is proportional to the linear velocity and the self-induction per unit of length. The resistance of the ring is easy to obtain by rotating the ring under the action of the earth's magnetic force. One great difficulty was to get an arrangement in which there should be no reaction between the magnetic field and the ring itself. Field magnets were therefore used instead of the magnetic force of the earth, and consequently there could be no reaction from induced magnetism in the earth, owing to the magnetisation of the ring. It was therefore assumed that the initial magnetic field was constant. That is the difficulty—to determine the reaction between the magnetism of the ring, and the induction which would be occasioned on the field magnets. A number of experiments were tried, and it was found that as far as the experiments went there was an increase of resistance owing to an increase of velocities, and that it was approximately proportional to the increase of velocity, and that it was approximately equal to what it was calculated it should be. It was found that the increase of resistance was a little larger than this amount; but it is almost impossible when turning a Gramme ring rapidly to prevent the jumping of the brush upon the commutator. The result of the experiments, however, led to the conclusion that the increase of resistance was to be calculated upon, and that there was a reason for not running dynamos any faster than a certain amount; that there was a certain velocity which it was well to not drive them at, because this internal resistance varied as the linear velocity of the ring. You can get over this resistance due to self-induction by constructing what are called unipolar dynamos.

First of all I may say something of the effect which Prof. Rowland called your attention to—the effect of size upon this apparatus. In the first place the angular velocity is inversely as the square of the linear dimensions. Therefore the linear velocity will vary inversely as the linear dimensions. Consequently the effect due to this linear velocity would be less in the larger machines, not more than half that of a machine of half the size.

The effect of the self-induction per unit of length of the ring will be independent of the size of the machine. For the coefficent of self-induction is in general proportional to the linear dimensions of the system. The coefficient of self-induction is in general of the dimensions of a line, and, therefore, the self-induction of a unit per length will be independent of the dimensions of the system. Consequently you will get this : the increase of resistance is inversely proportional to the linear dimensions. Inasmuch as the increase of resistance will be inversely proportional to the linear dimensions, and inasmuch as the current varies as the linear dimensions, you have that the loss of power will be directly proportional to the linear dimensions.

The whole power is directly proportional to the linear dimensions, so that the proportion of the power lost to the whole power will be constant and independent of the size of the machine.

It is possible by constructing what are called unipolar dynamos to get over this loss owing to the self-induction. For instance, take the simplest case of Faraday's experiment with a magnet rotating. In that case there is evidently no sudden change in the current. The current is gradually diminished. There is no sudden alteration in the current : it is a uniform action, and, consequently, there will be no loss in this case, so far as I can see, owing to the increased velocity at which it goes. You will have here the self-induction of the current diminishing, and helping to increase the current at some other point. Hence there will be no loss due to the stopping and starting. Some people find difficulty in understanding how there can be no loss in an opposing force. It is the case of a crank running upon a wheel ; there is no loss owing to the reciprocation of the crank, the mechanical force that is required to start it at one end being given out at the other end. But in this case it is like a bad crank, which joggles upon its bearings ; and in this case there is a loss by the reciprocation of the action.

In this case we have the loss due to short breaks, and in that case there would be a loss, owing to the increased pressure of the crank on the bearings. In every check of the crank there will be a loss, and hence in this case there will be loss, owing to the fact that you have to change the current from one direction to the other. But in this case of the unipolar machine it is like a crank axle in which the motion goes on uniformly without any unnecessary joggle round and round. Therefore there is no loss due to self-induction.

It is possible, also, to arrange Gramme armatures in such a way that there would be no loss owing to this cause, by arranging them so that there shall be a number of different brushes, and the current divided between these different brushes. The easiest way in which I can explain this is by taking the case of two Siemens armatures, the current being divided between them, part to one and part to another. Now, it is evident that it is possible to arrange the brushes in a certain position so that the brushes themselves shall make a contact in this armature at the time when the electromotive force is exactly the same as the electromotive force that is required to be sustained between those two wires, whereby the potential at the ends of the armature is the same as at the ends of the two brushes, the potential of the brushes being kept up while this contact is being made at the armature, so that you can make the contact with the brushes when the potential of the brushes is the same as the potential of the armature, and therefore there will be no tendency for the current to start or stop at that time through the armature, and therefore it must start gradually. On the other hand it is evidently possible to arrange the electromotive force so that at the instant of turning it shall be able to stop the current in this direction, but not to drive the current in the opposite direction ; thus driving the current in this direction during a part of the revolution and in this direction during another part. During the intervening time, it is evident that there will be some instant in which there is no current running in this armature. If that is the moment which is chosen for breaking the current there will be no spark, owing to the self-induction at that time. So that by making the contact between the brushes and the corresponding commutator at the time when the potential of the brushes is the same as the end of the armature, and when there is no current running in that coil, you will necessarily have a machine in which there is no loss in this way owing to the sudden breaking and stopping of the current.

I would just like to say something in connection with the question that Prof. Rowland mentioned upon another point, and that is, that in a large dynamo you have got an obvious advantage ; that it is possible to make the clearance very much less in proportion than in the small dynamo.

You can make the angular distance between the armature and the poles very much smaller in proportion in the large dynamo than in the small one, owing to the possibility of making the clearance much less. Another advantage in the large dynamo, on account of the electromotive force being nearly the same in the two dynamos, you may make the proportion between the area involved in the insulation and the area of the conductor smaller in the large dynamo than in the smaller one. It would not be necessary to make the area of this armature proportional to the linear dimension, consequently you can have a much larger conductor in proportion in the large than in the small dynamo ; so that that is an additional advantage you can have in the large dynamo.

With reference to the centrifugal force. While I was waiting, I worked out a little point that I would like to draw attention to, and it was with reference to the Gramme ring. I have come to the conclusion that the tension of the material and the wires, per unit of area, will vary directly as the square of the linear velocity; and that therefore the velocity being inversely proportional to the linear dimensions, the tension per unit of area will vary inversely as the square of the linear dimensions ; and that therefore you may drive the larger Gramme ring considerably faster than you could the smaller without any danger of its flying to pieces. I think the tension of the ring is exactly proportional to the square of the linear velocity. But I am not sure. I am sorry that this is all I can give you upon the subject.

(To be continued.)

NOTES.

Electric Lighting.—We understand that the Stanners Close Steel Company has decided to extend its electric light plant by the addition of some 10 Brush arc lamps, six of which are now being placed. As this firm has been employing the light for some time, the present extension speaks well for its success. The work is being carried out by the Hammond Electric Light Company, Limited.

Mr. Joseph Fearfield, of Stapleford, has, with the practical assistance of his son, Mr. J. P. Fearfield, introduced a most successful installation of electric lighting into his factory. The machines employed are three Gramme dynamos. The largest, capable of supplying 100 20-candle lamps, is used for nearly all the lights in the whole of the large factory, including clocks, passages, press rooms, &c. Another machine, capable of lighting 30 20-candle lamps, feeds the lights in the smaller factory at the rear of the main building. The largest machine is working at 50 volts, a current of 110 ampères being used when 93 lights are on. The smaller machines are working at 100 volts, and are only producing from 16 to 18 ampères of current. The whole of the dynamos are compound shunt-wound. Several lamps are in use, including Swan's, Edison's, Oppermann's, Lane-Fox's, British Electric Light Company's, Swinburne's, and Woodhouse and Rawson's. Mr. Fearfield began practically to use the lights on the 10th of August last, and, up to the present, only two lamps have really given out in working, and three globes have been broken by pure accident. In a room 75 feet by 45 feet, and 14 feet high, 24 lamps give a better light than 36 No. 5 gas burners used to do. The motive for the change is principally to provide a safeguard against fire-risk, which is now minimised, and also to free the atmosphere in the workrooms from the deleterious fumes arising from gas-lights. From the various data that could be obtained, he did not expect the change to be an economical one, but he now finds that, after deducting the slight extra cost of slack, and a little extra attention on the part of the engine-driver, the expense of lighting by electricity will not be quite one-third of the cost of lighting by gas ; indeed, so satisfied is he with the new mode of illumination that he is having two larger machines fixed, and a new set of engines built for the purpose of lighting up the remainder of the lace machines, his own dwelling house, the boiler-houses, &c. Mr. Fearfield, jun., has carried out the entire work of fixing, &c., even to the finishing of the machines, and, being a member of the Institute of Mechanical Engineers, has had an application from the council to kindly throw open the works for the inspection of the members during their approaching visit to Nottingham ; but this was thought somewhat impolitic on account of the lace machinery, and the honoured visit has been respectfully declined.

The works for lighting Temesvar, Austria, by electricity, are now completed, we learn from the Berlin correspondent of the *Times*, and on the 26th ult. all the street lamps were burning with half, and on the 27th with full power. The result is said to have been entirely satisfactory. On Saturday last the gas was to be done away with, and the illumination given exclusively by electricity. The official tests will begin on the 15th of November and continue till the 1st of December, on which day, if the tests are satisfactory, a contract will be signed with the company concerned (the English International Electric Company).

On examination of the Capitol at Washington, where the electric light has been used, states the New York *Electrical World*, it has been found that the building presents a most untidy appearance by reason of the billions of insects which have been drawn thither by the brilliancy of the lights, and whose skeletons are either hanging on the walls, held fast by a death grip, or are piled up in heaps all over the recesses of the roof.

Every effort is being made by the contractors to complete the plant and machinery necessary for generating the electric light at the Royal Courts of Justice by Monday, when the Lord Mayor and Sheriffs will go in procession up the central hall in order that his lordship may be received and sworn in before Her Majesty's judges. Latest accounts, however, point to the improbability of their succeeding, and in all likelihood the imposing ceremony will proceed in semi-darkness. Mr. Justice Hawkins expressed great dissatisfaction with the lighting arrangements on Thursday, ordering the lamp alloted to him to be removed as worse than nothing. Next summer, he said, when they would not want it they would have a little light.

The Swan Company for Isolated Lighting of New York, about a month ago, started 500 incandescent lamps among the paper mills at Bellows Falls, Ct., with a thoroughly arranged plant. Contracts with other mills have already been made, which will carry the number over 750 lamps.

Hachette et Cie, the large firm of publishers and booksellers in Paris, with branch in London, have had their folding and binding departments in the Rue Stanislas lighted by incandescent lamps for two years past. So satisfied are they with the results, that they have now arranged to extend the electric light to the whole of their establishment, and have ordered 350 Edison lamps.

The following circular has been forwarded by the Secretary of the Universal Exhibition of Antwerp to the principal business houses engaged in electric lighting :—"The Executive Committee of the Exposition Universelle d'Anvers has decided that the rooms and gardens of the Exhibition shall be lighted by electric light. We have the honour to invite you to lend your assistance for this lighting and to quote the price which you will charge for installing electric lamps (besides those which you will be disposed to place gratuitously as an exhibit). The committee will be happy to see the greatest possible number of manufacturers participate in this lighting. In order to act upon the tenders and to arrange the distribution of the lighting among the various exhibitors, the committee will take the opinion of a sub-commission consisting exclusively of persons having no connection with any electric lighting company, and who alone will know the proposals made to the committee. The members of this sub-commission are Messrs. Devy, Dumont, Evrard, Gérard, L. Gody and Wijbauw, and it is under the presidency of M. E. Rousseau. The price should be given per carcel-hour for each intensity of lamp, from arc lamps of the greatest power (two of which are to be placed in the lanterns at the entrance) to incandescence lamps of every pattern. The measurement of the intensity of the lights will be taken naked, at the most favourable angle, with the Bunsen photometer and according to a mean furnished by three observations. The price should be given on the supposition of free motive power, and the power required for each type of lamp should be indicated. Gas for gas engines serving for lighting will be reckoned at 16 centimes the cubic metre. The lighting will last each evening until 11 o'clock, three times per week, from the 1st of June to the 31st of October. All the conductors will be ærial and insulated. In order to take account of the variable expenses for the establishment and for the mean value of the conductors after taking down, *one franc per kilogramme of copper* employed will be paid. The prices will include all the expenses of installation (including belts and pulleys); the cost of carriage according to the tariffs of the various countries; the maintenance of the machines; the fixing up and taking down of conductors; the fixing, maintenance and superintendence of the lamps. The supplying and erecting of the posts and supports for the lamps and cables will be at the expense of the administration of the Exhibition. A scale of special charges will regulate questions of detail, and you will be informed in sufficient time. Tenders must be sent in *before the 15th of November*.

Messrs. Andrew Usher & Company, distillers, Edinburgh, have adopted electricity as the illuminant of their extensive premises. While several advantages over gaslight are claimed for the electric light, that upon which most stress is laid, in the present instance, is its safety against the origin of fire. The installation has been carried out by the Anglo-American Brush Electric Light Corporation, Limited. A calculation has been made that the introduction of the electric light will effect a saving of 33 per cent.

The streets of Wycombe were brilliantly illuminated by the electric light last Tuesday, when the Prince and Princess of Wales passed through the town on their way to Lord Carington's residence, Wycombe Abbey.

Mr. Altman, Chairman of the Streets Committee of the Commissioners of Sewers, recently stated that an application had been received by the committee for electric lighting, which, with modifications, would probably be accepted; he saw no reason why the greater part of London should not be illuminated by the electric light.

Nine Brush lights, each of 6,000-candle power, are used at the Hell Gate Lighthouse, to illuminate the entrance to New York harbour, and vessels 40 miles out on the Sound have distinguished the steady glow. There is no difficulty in discerning the treacherous rocks in the once dangerous strait, and vessels can pass as readily at night as when the sun is shining.

Mr. F. C. Phillips, of 2, Victoria Mansions, Westminster, has during the past week been lighting the chrysanthemum show of Messrs. Cutbush and Son, at Highgate, by means of four of his new 10-ampère arc lamps.

Telephony by Underground Conductors.—Mr. T. D. Lockwood writes to the *Scientific American*, confuting statements made in that journal in relation to a paper read by him at the recent Electrical Conference at Philadelphia. The statements were that "Prof." Preece believed that wires may be efficiently and economically buried; that at a recent meeting of the telephone managers a paper was read by an employé of the American Bell Telephone Company, whose duty it is to keep the lines in running order, with the object of showing that telephone lines could not be efficiently operated underground; that at the conclusion of the reading "Prof." Preece took the writer severely to task for the incorrectness of his conclusions, remarking that "if that was the result of his investigations, he must sadly have neglected his business"; and that results with underground telephone wires were more than encouraging. "In the first place," says Mr. Lockwood, "Mr. Preece lays no claim to a professorship—he is chief electrician of the British telegraphs, and a Fellow of the Royal Society. Second. The paper was not read at any meeting of the telephone managers, but at the afternoon session of the fourth day of the National Conference of Electricians. Third. The duty of the employé of the American Bell Telephone Company who read the paper (myself) is not to keep the lines in order. Fourth. Not being actively engaged in the business of telephonic communication, the A. B. T. Co. has no lines. Fifth. The object of the paper was not 'to show that telephone lines at least could not be efficiently operated underground.' The subject then under consideration by the conference was: 'Induction in telephone lines, long line telephony, and underground wires,' and the paper, related to the subject as a whole, was prepared by request of the U. S. Electrical Commission, and only incidentally touched on underground telephony. Sixth. Prof. (?) Preece did not take the writer severely to task for the incorrectness of his conclusions, although he freely criticised his premises. Seventh. Although Mr. Preece did make the remark cited, it was by no means with reference to underground wires, but merely referred to the omission from the paper of several methods for preventing induction which had gone into use in England. Eighth. The inference that Mr. Preece held the opinion that telephones could be worked for considerable distances underground is not warranted by the facts. The paper stated that telephone wires could not be successfully and commercially operated underground for a greater distance than 12 miles, and Mr. Preece fully concurred in that statement both at the Montreal meeting and at the Philadelphia conference; while the remark that 'even telegraph wires are constructed underground at four times the expense of overhead wires, while they are but one-fourth as efficient,' was made by himself not over a year since in a lecture before the Society of Arts in England. Ninth. It is not true that the results so far obtained with underground telephone wires have so far been encouraging. On the contrary, they have been discouraging. The articulation invariably becomes sluggish when the underground conductor exceeds two miles in length, and this effect is greatly accentuated when an overhead line of several miles in length is connected with the underground line. Increasing the sectional area of the conductor aids materially in overcoming the sluggishness."

New Telephone Company in Canada.—The Dominion Telephone Company has been organised at Montreal to manufacture and use the James Wright patents. The rentals to be charged are $55 the first year, and $25 subsequently; $30 of the first year's charge purchasing the instruments, and the $25 being for maintenance. A new feature will be the supply of private switches for 5, 10, and 20 wires. There are at present in operation over 100 sets of instruments in the townships, and over 60 miles of line have been erected, and 100 miles are in course of construction. The company expects to commence operations in Montreal in the spring. The invention was made in March by Mr. Wright, and since then the instruments have been in use, it is stated, without interference from the Bell company.

Telephone Extension in the North.—The Ecclesfield and Bradfield Highway Board, on Monday, gave permission to Messrs. Tasker and Sons, Sheffield, to erect telephone posts and wires from Wardsend Steel Works to Messrs. Moss and Gamble's works, Wadsley Bridge, and from the Asylum to Deepcar, on payment of 2s. 6d. per mile per annum.

Death of Mr. Fawcett, M.P.—Very great surprise and truly sincere regret, we are sure, will be felt by our readers when they learn the intelligence of the sudden death, on Thursday last, of the Right Honourable Henry Fawcett, M.P., her Majesty's Postmaster-General. The *Morning Post* in commenting thereon says: "The position which that officer holds in the Administration is not one which brings him into collision with the opposite party; but in his case much admiration was excited by his beneficial administration of his important office. He has passed away at an early age (51 years) for a statesman of our time, but not without having left his mark on the department over which he had been chosen to preside."

Australian Telegraphy.—Western Australia is engaged in constructing a line of telegraph along about 700 miles of its coast line, from Northampton to Roebourne, the objects being to establish speedy means of communication with the settlements near Roebourne; and to induce an English company to run a cable to North-West Cape from Java. The first and most difficult section, from Northampton to Hamlyn Pool, a distance of 140 miles, has just been completed: the poles are of iron and are in two sections.

Telegrams to Canton.—The Great Northern Telegraph Company announces that the rate to Canton has been reduced to 8s. 9d. per word from the 1st instant. A telegraph station has been opened at Swatow, for which place messages can now be accepted for transmission by this company's lines at the rate of 10s. per word.

The Telegraphs of British India.—Mr. H. G. James, Postmaster-General for British India, is at Berlin, studying the postal and telegraph institutions of the German Empire.

American Telegraphs.—Western Union stock, says the *Finansial News*, when Mr. William H. Vanderbilt went out of the control and turned it over to the tender mercies of Mr. J. Gould, was selling at about $80 per share. It has since been doubled, and now sells at about $60 per share, or at the rate of $120 for old stock. Since Mr. Gould obtained control he has sold out pretty much everything of value to pay unearned dividends, and the treasury is as bare as a billiard ball.

Incandescence Electric Lamps.—We would call attention to the letter of Mr. Trotter on this subject in our correspondence columns. Perhaps others of our readers may have some information to give bearing thereon, the matter being one of much importance to all connected with the practical operations of electric lighting.

Use of Submarine Cables in Time of War.—The first issue of the *Admiralty and Horse Guards Gazette* contains an article upon this subject, in which it is urged that no squadron should be without a fleet telegraph-ship in time of war, and the various advantages, such as the ability to tap cables and transmit information in mid-ocean, to intercept the enemies messages, &c., are pointed out.

The Invention of Electric Telegraphy.—The Premier of Victoria has written to the Agent-General regarding the claims of Mr. E. Davey, surgeon, of Malmsbury, to be one of the earliest inventors of the method of using dynamic electricity for telegraphic purposes, stating that Mr. Davey is genuinely entitled to the gratitude and consideration of the whole civilised world, and mentioning that £100 has been placed on the Victorian Estimates this year. As an initiatory step, he requested Mr. Murray Smith to support the claim to be made on the Imperial Government by the Royal Society of Victoria through the Royal Society of London, on behalf of Mr. Davey.

Telegraph Clerks and Sunday Labour.—At a meeting of telegraph clerks at Newcastle last Saturday, a resolution was unanimously passed regretting the Postmaster-General's refusal to grant payment for all Sunday work in the provinces, while such work performed in London is paid for as overtime; and stating that so long as this invidious distinction exists deep dissatisfaction will continue to pervade the provincial staff. It was stated that in the service of the companies, and for some time subsequent to the transfer of the telegraphs to the Government, Sunday work was paid for as overtime.

Electrical Tramways in Blackpool.—The Blackpool Town Council has decided to enter into an agreement with Mr. Holroyd Smith to work the electric tramways in the borough for seven years, paying the corporation 6½ per cent. on the money it invests in laying down the lines. The tramways are to be in working order by June 1st next year.

Overhead Wires.—The Clerkenwell Vestry has appointed five delegates to join the conference at the Charing Cross Hotel, on the 17th inst., to consider the desirability of memorialising the Government to confer upon local authorities control over the erection and maintenance of overhead wires.

The Society of Telegraph Engineers and Electricians.—On Thursday next, at 25, Great George Street, the following papers will be read:—1. "On the theory of alternating currents, particularly in reference to two alternate current machines connected to the same circuit," by J. Hopkinson, F.R.S., Member. 2. "An account of experiments with alternate current machines," by Professor W. Grylls Adams, F.R.S., President.

The American Electrical Exhibition.—The exhibition to be held in Boston from November 5th to January 3rd, 1885, will include the following sections:—The production of electricity, electric conductors, measurement, application of electricity (high power), application of electricity (low power), terrestrial physics, historical apparatus, special novelties, and electrical literature. It is intended to make the exhibition complete and comprehensive in every particular, and to exceed in novelty any that has ever been held in New England.

Electricity Without Motive Power.—Last week, at the Manchester County Police Court, Samuel Harrison, a middle-aged man, of Gorton, was committed for trial on a charge of obtaining £180 by false pretences from James Edward Frith, a baker, who said the prisoner had completely deceived him by representing that he had patented an invention for producing electric light without any engine or motive power, which would run tramcars and drive engines, and could be put in a meter and turned on like gas. The evidence betrayed extraordinary credulity on the part of Frith and others.

Electro-Magnetism for the Cure of Diseases.—A "Society for the Extension of the True Electro-Magnetic Cure of Disease" has been formed, one of the objects being to establish in London, and elsewhere, hospitals where sufferers, especially the poor, may be provided with electro-magnetic machines and suitable attendance. The *patients* in these establishments will have our sympathy.

Electrical Engineers' Catalogue.—Following the example of other enterprising manufacturers, Messrs. A. R. Sennett & Co., electrical engineers, of Hatton Garden, have heightened the value of their catalogue by including interesting descriptions and explanations of the various requisites for an installation of electric lighting.

Gas Engines.—A reprint from the *Mechanical World* of the articles on "Gas Engines: their theory and construction," by Mr. C. F. Budenberg, B.Sc., has been forwarded to us. It appears to be a most complete history and description of gas motors.

Processes of Gold Extraction.—The paper on "Some modern processes of gold extraction," read by Mr. Rowland J. Atcherley, Ph.D., F.C.S., before the Balloon Society on October 31st, has been printed and published in pamphlet form. The treatise is interesting, and the latter part deals with the most recent methods of extracting the precious metal from its ores by means of electricity.

Trial Trip of the "Mackay-Bennett."—This steamer, which has been built by Messrs. John Elder & Co., at Fairfield Works, expressly to serve as a cable-repairing ship for the Commercial Cable Company, made her trial trip on Friday week in very stormy weather. The trial was in every way satisfactory to the officials who were present on behalf of the company. The bow and stern paying-out and picking-up gear were constructed and fitted up on board by Messrs. Elder for Messrs. Siemens Brothers & Co., to whose works in London the *Mackay-Bennett* proceeded on Saturday to receive her cable, electric apparatus, &c. The *Mackay-Bennett* was built under the special superintendence of Mr. St. Clare Byrne, of Liverpool.

Death from Lightning.—At a recent congress of German medical men, held at Magdeburg, a paper was read on the effects of lightning stroke on human beings, and the author showed that when the lightning discharge passed through the skin its passage was much easier, that is to say the internal organs are much more conductive than the epidermis. This fact, says *Engineering*, was pretty well known, but it is not so well known that the brain and spinal cord are apparently conductors, and hence a lightning stroke on the head does not materially injure the brain beyond shattering the nerves, and causing temporary derangement. Most persons struck by lightning do not remember anything about the stroke; but others describe a sensation such as would be caused by their being struck a heavy blow, and some have likened the shock to what they would be supposed to feel if torn into small pieces.

Prevention of Railway Accidents.—A Swedish engineer has perfected an invention for preventing railway accidents at points and swing bridges by means of an electrical machine placed in the telegraph office of the station, and connected by wire with two bells on a pole at the points in question. The engine of the train is fitted with a spring disc, seven inches in diameter; this disc makes contact with the pole as the train passes, and the bells are thereby made to give warning to the driver of an approaching train or one following, and the passage of the train is indicated at the nearest station.

Fire at the Silvertown Telegraph Works.—On Monday morning, before daybreak, a fire broke out at the works of the India Rubber, Gutta Percha, and Telegraph Works Company, at Silvertown, and burned furiously for some hours. It originated in a rubber store, which was burned out, whilst the remainder of the building and its contents were damaged by fire and water. Two steamers and a manual engine were in attendance from the West Ham fire brigade station.

Capital Punishment by Electricity.—A bill has been introduced into the Vermont Legislature by the opponents of capital punishment by hanging, providing for the execution of murderers by electricity.

Proposed Electric Railroad over Brooklyn Bridge.—The Knight-Bentley Company has submitted a proposal to work the railroad on Brooklyn Bridge by electricity, the present cable system giving great dissatisfaction.

International Exhibition of Inventions.—The number of applications already sent in for next year's exhibition represent more than twice the available space.

Underground Telegraph Wires.—The Stafford Town Council has granted permission to the Post Office authorities to lay down pipes for telegraph purposes.

Engineers' Employment Register.—Mr. James P. Maginnis, M.I.M.E., has opened an employment register at 23, Queen Anne's Gate, Westminster, S.W., for the benefit of engineers and others requiring assistants; and also for engineers, works-managers, and draughtsmen requiring situations. The method pursued appears to be satisfactory, and the rates of commission charged on the first year's salary are moderate.

Transfer of Business.—In order to devote their attention more fully to the development of the electric light, telegraph and telephone, and electric signal business, Messrs. Paterson and Cooper, of 76, Little Britain, E.C., have disposed of that portion of their stock and business relating to educational, philosophical, and experimental apparatus to Messrs. J. T. Mayfield & Co., of 41, Queen Victoria Street.

Obituary.—We regret to record the death of Mr. Frederick John Smith, an occasional contributor to our columns, which occurred this week at Taunton, where he was highly esteemed for his munificent works.

Torpedo Warfare.—The 14-gun corvette *Pylades*, launched at Sheerness on Wednesday, is fitted with electric machinery for the discharge of torpedoes.

Paris Exhibition, 1885.—The International Exhibition of Manufactures and Processes at Paris will be opened on the 23rd of July, closing on the 23rd of November, next year. The Exhibition is under the patronage of the Minister of Commerce, the Minister of Public Instruction and Fine Arts, and the Minister of Public Works. It will be held at the Palais de l'Industrie, Champs Elysées, and is to include a science annexe and a fine art annexe. The London offices are at 1, Castle Street, Holborn, where applications for the remaining available space and all communications from British exhibitors should be addressed to Mr. Edmund Johnson, Commissaire Delégué.

Institute of Mechanical Engineers.—At the meeting of the members of this institute at Nottingham on Wednesday, Mr. Timmis, of London, read a paper on the application of electro-magnets to the working of railway points and signals, claiming that with the system he advocated the distance of the signals from the box was immaterial. The cost was slight, the chance of error was avoided, and an infallible check could be obtained as to the working of the magnet.

The "Great Eastern."—The syndicate which on behalf of the Commissioners of the New Orleans Exhibition has chartered the *Great Eastern* steamer, took possession of the vessel at Milford Haven on Thursday. It is proposed to engage a crew of about 300 men, and Captain Halpin, who formerly commanded the great steamer, will probably again take charge of her.

NEW COMPANY REGISTERED.

Primary Battery Company, Limited. — Capital £6,300, in £5 shares. Registered office, 12, Princes Street, Hanover Square. Objects : To adopt two agreements, dated respectively 16th July and 22nd of August, between Thomas John Jones and Desmond Gerald Fitzgerald, of the first part, David Armytage Davis, of the second part, and Josiah G. Stapleton, of the third part ; and to produce and supply electric light and power by means of primary batteries. Signatories (with 1 share each), *T. J. Jones, telegraph engineer ; W. Ninnis Tiddy, telegraph engineer, and D. D. A. Davis, of 12, Princes Street, Hanover Square ; *J. Gloch Stapleton, 47, Lee Terrace, S.E. ; *W. Arbuthnot, 15, York Buildings, Adelphi ; D. G. Fitzgerald, electrician, 6, Akerman Road, Brixton. Qualification for future directors : 70 shares, or £350 stock. The company in general meeting will appoint the remuneration of the board. Registered 30th ult., by Walls, Abbott & Martin, 11, Queen Victoria Street.

* Signatories whose names are preceded by an asterisk are also directors.

Change of Office.—The registered offices of Edison's Indian and Colonial Electric Company, Limited and Reduced, are now situated at 3, Great Winchester Street, S.E.

CITY NOTES, REPORTS, MEETINGS, &c.

Brazilian Submarine Telegraph Company, Limited.

THE 22nd half-yearly meeting of this company was held at Cannon Street Hotel on Friday, the 31st ult., the Right Hon. Viscount Monck presiding. The directors' report was given in the REVIEW of the 25th ult.

The Secretary, Mr. Richard Collett, having read the notice convening the meeting,

The Chairman said last year he was obliged to give rather a dismal account of their affairs; their cables were interrupted, their business was suspended, and their receipts were annihilated for the moment. He was very happy to tell them that on the present occasion he could completely reverse that picture. Their property in every respect was in an admirable state. They had a new cable laid, and the old cable was known to be in a good condition, not merely from scientific tests, but from actual inspection. Of the three breaks which had occurred in the old cable, they were entirely unable to account for the cause of one: the electricians had suggested that it might have been connected in some way with the extraordinary volcanic action which prevailed during the year 1883 all over the world, and they could see no other means of accounting for it. The other breaks had been occasioned in the usual way by abrasions in consequence of friction with a foul bottom. With regard to the income of the company, taking the six months from January to July, there were four weeks in March when the cable was suspended—that was before the second cable was completely laid—and of course there was a diminution of receipts during that period. Comparing with the similar period of the year before, and allowing to the period of suspension receipts equalling those of the weeks immediately succeeding, the income of the two periods was within £100 or £150 of the same amount. Taking the circumstances into account, he considered that this showed a growth in the receipts; because contemporaneously with the reduction of tariff there was competition for the carriage of messages to Chili and Peru by what was called the West Coast route, and their receipts having continued equal notwithstanding these disturbing circumstances, they might assume that there would have been an increase without them. The expenditure of the two half years showed an increase of £641 for the half year just passed; but in that expenditure was included the cost of the repair of the cable in March last, £760, so that in point of fact the expenditure was £120 less than the corresponding half-year. The general expenses in London showed a decrease of £227, and the working expenses at stations an increase of £172, due to the provision of new instruments. It was announced last year that they were about to duplicate the cable. He frankly confessed that he gave his adhesion to that policy with some misgivings; he was loth to sink in the sea the results of their reserve fund until it became necessary to have a second cable for the performance of their work. One cable was abundantly sufficient at the time, and therefore he was at first rather opposed to the duplication. However, he gave way to the arguments of his colleagues, and he was very glad now that he did so, for events had since entirely justified their policy. When he announced that they were about to duplicate the cable, he told them that they could not do it altogether by means of the reserve fund; he said they would require £250,000 in addition. If they looked at the report they would see that he greatly overstated the amount, as only £150,000 in addition to the reserve fund were required. He laid some stress on the point that they would be able to renew the cable without increasing their share capital. By increasing the share capital they would be introducing what was practically a permanent participant in their dividends, an element which must diminish their receipts in the dividends, and an element practically impossible to get rid of; but by raising the money by means of debentures they merely had a debt which they could pay off at some future period, and which would not be permanently operative in diminishing their receipts. He did not like to give too rose-coloured a description of affairs, but he hoped and believed that they would be able at least to pay the shareholders what they had been in the habit of receiving for the last four or five years, and that they might be able to make good any future breaks without either drawing on the debenture capital or raising a loan of any kind. He trusted that the mode in which the directors had managed the business during the last 12 or 13 years had been satisfactory. They began by giving a comparatively shall dividend, and when they found that they could safely do so they increased it to a larger amount; and they would go on in the same course, consulting not merely the temporary and fleeting interests of the shareholders, but the permanent and lasting good of the company. He moved that the report and accounts to the 30th June, 1884, be received and adopted, and that a dividend of 3s. per share, making, with interim dividends, a total of 6 per cent. for the year, free of income tax, be now declared.

Sir James Anderson seconded the motion.

Mr. Griffiths enquired as to the receipts of the last three months.

The Chairman said they had been very similar to those of the six months previously; they were almost identical with the corresponding three months of last year. There was a slight diminution, as in the other case, but at the same time the months of October, November, and December were the three best they had, and probably the average receipts of the six months would be superior to the average of the six months previously.

The resolution was then put and carried unanimously.

Sir Thomas Fairbairn and Mr. H. D. Gooch were re-elected directors, and the reappointment of Mr. Henry Dever and Mr. John Gane as auditors, at a remuneration of £50 each, was agreed to.

A vote of thanks to the Chairman and other directors terminated the proceedings.

Oriental Telephone Company, Limited.

THE directors of this company have issued a circular referring to a movement amongst a section of the shareholders which has for its object the appointment of a committee of investigation and a change of the directorate. The circular states :—

" Having reason to believe that the agitation is wholly actuated by feelings of animosity towards some of the members of the board, the directors feel that they are justified in requesting the shareholders to withhold any promises of adhesion to any agitation until they have at least had an opportunity of hearing both sides of any question that has been or may be raised. The directors are ready to meet the shareholders whenever occasion arises. Meanwhile they are prepared to give every shareholder the fullest information they themselves possess, with perfectly free access to the records of the company. The revenues of the company and of its affiliated companies have materially improved since the last report, whilst the expenses have been considerably diminished;" and in conclusion cautions the shareholders against blindly sacrificing their property.

The Insulite Company, Limited.—A special meeting has been called for the 17th inst., for the purpose of confirming the following resolutions, which were passed at a previous meeting:—"1. That this company hereby adopts the agreement, dated October 22nd, 1884, made between this company, of the one part, and the British Insulite Company, Limited, of the other part, being the agreement mentioned in the notice of this meeting (dated October 10th) as providing for the transfer to the British Insulite Company, Limited, for the assumption by the last-named company of this company's assets and liabilities. 2. That it is hereby required that this company be wound up voluntarily."

Telegraph Construction and Maintenance Company, Limited.—Second Bonus Trust.—Coupon No. 19 on the certificates issued under the trust deed of the 12th March, 1875, at the rate of 1s. 4d. per £5 certificate (nominal value) is payable at the offices of Messrs. Barclay, Bevan & Co., No. 54, Lombard Street. Further supplementary sheets containing six coupons (numbered from 19 to 24) may be obtained by certificate holders upon production of their certificates at the offices of the company, 38, Old Broad Street, E.C.

The Eastern Telegraph Company, Limited.—This Company announces the payment of interest for the half-year ending 31st inst., on its 4 per cent. mortgage debenture stock.

TRAFFIC RECEIPTS.

The Cuba Submarine Telegraph Company, Limited. The number of messages passing over the lines of this company during the month of October was 2,588, estimated to produce £2,200, against 2,814 messages, producing £2,744 in the corresponding month of last year. The receipts for the month of July, estimated at £3,200, realised £3,219.

The West India and Panama Telegraph Company. Limited. The estimated receipts for the half-month ended the 31st October, are £2,468, as compared with £2,290 in the corresponding period of 1883. The July receipts, estimated at £5,865, realised £5,932.

The Western and Brazilian Telegraph Company, Limited. The receipts for the week ending October 31st, were £2,934; after deducting the fifth of the gross receipts payable to the London Platino-Brazilian Telegraph Company, Limited.

The Great Northern Telegraph Company. The receipts for October, 1884, £26,000; and from 1st January to 31st October, 1884, £234,680; for the corresponding months of 1883, £215,235; and for 1882, £203,575.

The Direct Spanish Telegraph Company, Limited. The estimated receipts for the month of Ootober, were £1,794, against £2,115 in the corresponding period of last year.

The West Coast of America Telegraph Company, Limited. The gross earnings for the month ending October 31st were £4,065.

NEW PATENTS—1884.

14296. "Laying, carrying and insulating electric telegraph and telephone wires and electric light and power wires." J. C. SELLARS. Dated October 29.

14377. "Electric apparatus for recording the circulation of trains on railways." B. J. B. MILLS. (Communicated by C. Diener and C. A. Mayrhofer.) Dated October 30.

14445. "Galvanic batteries." W. R. LAKE. (Communicated by F. Kübmaier and J. Wannieck.) Dated October 31.

14448. "Electric arc lamps." G. C. FRICKER. Dated November 1.

14476. "Electric batteries," E. G. BREWER. (Communicated by J. Cerpaux.) Dated November 1. (*Complete.*)

14485. "Galvanic batteries." G. BARON DE OVERBECK. (Communicated by F. Horming.) Dated November 1.

14508. "Secondary batteries." A. H. REED. (Communicated by E. Commelin, G. Bailbache, A. L. de Virloy and L. de Bousignac.) Dated November 3.

14547. "Universal safety incandescence electric lamp holder." S. J. COXETER, H. NEHMER. Dated November 4.

14548. "Electric batteries or elements." P. M. JUSTICE. (Communicated by F. Van Rysselberghe.) Dated November 4.

14553. "Manufacture of carbons for incandescent electric lamps." G. DAVIDSON, R. C. JACKSON, J. B. DUNCAN. Dated November 4.

14560. "Galvanic battery for use in surgical operations, &c." H. J. HADDAN. (Communicated by Electrotechnische Agentur Richard Blänsdorf.) Dated November 4.

ABSTRACTS OF PUBLISHED SPECIFICATIONS, 1884.

90. "Improvements in electric block signalling instruments." W. E. LANGDON. Dated January 1. 6d. The object of this invention is to provide a more ready and more easily operated and secure means for pegging over or fixing in any required position the electric commutator handle or tapper by which the fixed signals, such as the "line blocked" or "line clear" signals, are produced. To this end the inventor provides, in the place of the pin employed for pegging the handle over as at present, a small key or thumb piece, which is fixed in the neighbourhood of the handle, either on one side of the same, immediately over it, or in other convenient position, a small key having two or more positions, one a position of rest, the other a locking position; or two positions for locking, one to lock to the "blocked" side, the other to lock to the "clear" side or position. This key is formed of such a shape that a part or extended portion shall, when the key is depressed in a certain position, engage with a fixed part of the instrument handle or commutator, or with a piece fixed thereon when the said instrument handle is held in the required position for affording the permanent signal. To release the key from this position it is merely necessary to raise it or press it in the opposite direction, that is, into its position of rest.

234. "Improvements in the manufacture of incandescent lamps." J. H. GARDINER. Dated January 1. 6d. The inventor winds the fibre or thread to form the filament, whilst in a moist and flexible state, upon a cylindrical block to a spiral form with two straight central ends projecting in opposite directions. The fibre or thread is kept tightly distended by springs until it sets in the spiral form. The block is made in two parts, which separate to release the spiral. The filament is carbonised in the usual way. It is then if necessary plated by igniting it in the presence of hydrocarbon vapour to adjust the resistance, and for this purpose its ends are held in clips supported upon a glass rod. The ends of the filament are then set into terminals. The terminal consists of a platinum wire armed at one end with a cup or tubular socket of a size to admit the extremity of the filament, and the other end of the wire is turned simply into a loop. The wire is coated spirally with glass, and for this purpose it is clamped in a mandril on which is a screw thread. The mandril can be revolved by a handle in a bearing, which also permits of longitudinal movement. A spring piece bearing on the screw thread limits the longitudinal movement to that due to the advance of the screw thread, except when the spring piece is pressed out of the way. The platinum terminal having been clamped centrally in the mandril is advanced by turning the handle between the points of two blow-pipe flames, one on either side. When the platinum is thus heated a thread of glass is wound upon it so as to cover it completely, the even distribution of the glass being ensured by the forward movement of the platinum wire whilst it rotates, and winds the glass upon it. Two terminals thus prepared are then clamped upon a holder with the required interval between them, and the ends of the filament are inserted into the platinum cups or sockets. A good junction is made by the application with a brush of a suitable paste or paint.

8443. "Improvements in electric arc lamps." C. LEVER. Dated May 30. 6d. This invention relates to improvements in the electric arc light apparatus described in the specification of a former patent granted to the inventor on the 3rd May, 1882, No. 2092, and is intended to accomplish the following objects:—1st. To enable the electric arc lamps constructed according to the said previous invention to be more effectively worked in single circuit from a series wound dynamo-electric machine than that described in the said previous patent specification, and also to combine with the improved device an arrangement of the parts which will "cut out" the lamp should the circuit through the carbons be broken. 2nd. To provide improved means of regulating the "dash pot" in the lamp whereby compensation can be made for the different viscosities of glycerine or other similar fluid which may be used in the said dash pot.

CORRESPONDENCE.

The Economics of Incandescence Lamps.

You ask, in your leader of the 1st inst., "which system is the more economical in the long run, that in which the electrical energy is reduced to 2½ watts per candle-power, with a probable duration of 400 to 500 hours, or even less, as the life of the lamp, or that demanding 3½ to 4 watts per candle-power, with the certainty of the lamp lasting 1,000 hours?"

Dr. Fleming pointed out to me, some years ago, that the value of an incandescent lamp is its $\frac{\text{candle-power} \times \text{life}}{\text{watts} \times \text{price}}$. The cost per hour for power and lamp renewals is therefore

$$n\,e\,c + \frac{p}{l}$$

where e is the number of watts per candle-power, n the cost of a watt-hour, c the candle-power, p the price of the lamp, and l the life of the lamp in hours.

It is evident that the relation between the important variables, e and l, depend on the external circumstances, n and p.

We may take, as extremes, ½d. and 1½d. for the cost of an electrical horse-power of 746 watts, and for the prices of lamps, 2s. 6d. to 7s. 6d. With a 20-C.P. lamp, taking 4 watts to the candle, the cost of power per hour, at ½d per H.P., is 0·0535d.; and at a price of 2s. 6d., and a life of 1,000 hours, the renewal cost per hour is 0·03d., making a total of 0·0835. But if, other things being the same, the efficiency is raised to 2½ watts per candle, the cost of power becomes 0·0335, and this, subtracted from the total, leaves 0·05d. for lamp renewal; this is equivalent to a life of 600 hours. An exactly similar result is obtained with power at the high rate of 1½d., with a lamp at 7s. 6d.

But with power at the high rate, and with a lamp at 2s. 6d., the life comes to 333 hours; that is if a lamp with 4 watts to the candle has a life of 1,000 hours, and an increase of efficiency from 4 to 2½ watts (or 60 per cent.) be obtained, the life may be decreased from 1,000 to 333 (or 66·6 per cent.), *without altering the total cost of the system.* And it follows, that if the lamp still has a life of 1,000 hours, it *is worth three* of the lamps of lower efficiency.

In short, if the lamp is very expensive, say 7s. 6d., and power may be had for a mere nothing, all our attention must be devoted to prolonging the life, the efficiency being nugatory; but if we could get lamps at a fair and reasonable price, say 9d., and were forced to use some extravagantly expensive source of power, such as a primary battery, we should strive to attain a high efficiency, the life being only a question of the trouble of replacing the dead lamps.

There is, however, an aspect of the question which is, in general, more interesting than the mere structural differences of the lamps, and it applies to the whole system of incandescence lighting. The efficiency of any given lamp varies as some function of the degree of incandescence, or actual candle-power; the life also varies inversely as some function of the same, and it is possible that these two functions are identical. Be that as it may, the important question then arises—at what degree of incandescence (determined preferably by the spectrum) should the lamp be run in order that $n\,e\,c + \frac{p}{l}$ may be a minimum?

It is most unfortunate that we are still practically without any authentic information on these points. It is true that it is a simple matter of measurement to determine the former, but then it is only good for one class of lamp, while time only will tell the life; and then where is a lamp running day after day at the same volts, and under regular conditions of work?

It appears, then, that under average conditions, if we can get a lamp taking 2 watts per candle, we ought to be satisfied (relatively) with a life of 600 to 700 hours; and when the time comes, and it should have come by this time, for a shilling lamp, a long life would be a question of convenience rather than economy.

With the present high prices, the value of lamps of high candle-power is most important, and appears to be strangely neglected.

A noticeable feature in the more efficient kinds of lamps is their colour; the filament is lost in a blaze of white light which puts to shame the "4 watt red-hot hair pins."

Alex. P. Trotter.

7, Furnival's Inn, London, E.C.,
November 4th, 1884.

The Electric Lighting Act.

I am anxious to get information relative to the cost of obtaining a license from the B. of T. under the Electric Lighting Act. I shall therefore be glad to know the names of any local authorities or companies who have obtained licenses under the above Act.

In my case the license is required by the local authority for the district, so that there will be no opposition to meet.

Ohm.

Glasgow, *November 4th*, 1884.

Telephones.

In reply to your correspondent "S. R's." letter in the REVIEW of 1st inst., I shall review the several communications by "S. R.," published in the REVIEW, in regard to the "Mechanic Receiver," and his statement on September 20th, that "the first definite intimation in England of the great discovery of Bell was published in the *English Mechanic* on 11th August, 1876." I now repeat my statement in the REVIEW of 11th October, "that the first definite intimation in England of what "S. R." is pleased to call "the great discovery of Bell," was published many months before that date, and I submit the following indubitable evidence in support of my assertion.

The *first* publication of Bell's invention, *subsequently* claimed by him to be a system and instrument for transmitting speech, was on 7th March, 1876, in the United States "Official Gazette of Patents" of that date, and also the publication on that date of the full specifications and drawings of Bell's patent, No. 174,465, for "Improvements in telegraphy." The "Official Gazette" contained a drawing of Bell's original gold-beater skin instrument, and a great number of that "Gazette" was received in England by many patent agents in the month of March, 1876, and this "Gazette" was on public view at the British Patent Office on 3rd July, 1876, as I stated in my letter in the REVIEW of 11th October. The fact that the "Gazette" was in the possession of many patent agents is such evidence of its publication in England that the claim for the invention described in it, and subsequently included in Morgan Brown's (Bell's) patent, necessitated the disclaimer by Morgan Brown on 13th February, 1878. I hope "S. R." will now be satisfied that I did not "mistake the force of the word definite," and that the FIRST definite intimation in England of the great discovery of Bell was *not* published in the *English Mechanic* of August 11th, 1876, but *was* published in England in the U. S. "Official Gazette" many months before that date.

"S. R." quotes from my letter in REVIEW of 25th ult., my statement that "the first notice of Bell's invention was on September 15th, 1876, in *Engineering*." That statement was in reply to your correspondent "S.," who in his letter in REVIEW of October 4th wrote, "W. C. B. is not correct in saying that Bell's telephone was described in *Engineering* prior to the patent being applied for in this country," and in the REVIEW of October 18th "S." wrote "the first notice and description of Bell's original instrument was not published till December 22nd, 1876."

I know full well that Bell's original invention had been published in the *English Mechanic* on 11th August, and in the U. S. "Official Gazette" long before that date, and my reply was intended to refer to the publication in *Engineering* of 15th September, not to the publication in other papers. It must be clear that I misplaced the words "in *Engineering*;" the sentence should read, "the first notice of Bell's invention in *Engineering* was on September 15th, 1876."

In the absence of *positive* evidence that the U. S. "Official Gazette of Patents" of 7th March, 1876, was received in England in March or April, 1876, by many patent agents, there is the fact that that Gazette, which contained a description of Bell's invention, namely the gold-beater skin instrument, was proved before Mr. Justice Fry in the case of the United Telephone Company *v.* H. Cox-Walker, by Mr. Alison, Librarian of British Patent Office, to have been on public view in that office on 3rd July, 1876.

"S. R." and "S.," and your readers generally, will find that the statements in my letters to the REVIEW are correct.

W. C. Barney.

November 3rd, 1884.

Van der Weyde's Telephonic Instruments.

In your notice of exhibits at the Philadelphia Electrical Exhibition, you have added a very interesting and important leaf to the history of the telephone, especially important in its bearing on the Bell patents in Great Britain as well as in the United States.

It is as clear as day that Van der Weyde's telephone instruments exhibited in the drawings, figs. 11 and 12, in the REVIEW are in principle, construction and mode of operation the same as Bell's improved telephone now in use by the United Telephone Company. The question, no doubt, will arise whether these instruments were known or described in any public newspaper or book circulated in England prior to the date of Bell's patent, namely, the 9th December, 1876. On referring to the REVIEW of February 16th, 1884, page 145, your readers will read the following words in a letter signed by me: "I am strongly inclined to believe that evidence is in existence, and can be produced, which will establish the fact that there was published in England, in 1869, the following description of a telephone receiver:—'An electro-magnet in front of a brass diaphragm with an iron button attached to the centre of the diaphragm.' The original instrument is at present in the possession of a friend, and it talks as well as any Bell telephone."

When I wrote that letter I had in my possession photographs of Van der Weyde's telephone, precisely like all the drawings in the REVIEW.

I had at that time a newspaper published in the United States in 1869, which contained a description of this instrument; and I now repeat that I am inclined to believe that the newspaper referred to was largely circulated in England in 1869; I withheld the name of the newspaper, its date and place of publication in the U. S., for reasons satisfactory to myself. I send you some extracts from an affidavit made by Mr. Van der Weyde in a suit now pending in the U. S. Court, which will, no doubt, be an interesting supplement to your notice.

The deponent states: "I have read the patent of A. G. Bell, No. 174,465, March 7th, 1876. I recognise in the devices, fig. 7 (the goldbeater skin with the iron bar armature), an instrument substantially the same in its electrical operation as the receiving instruments used by me in 1869 and 1870."

"I have also read the patent of A. G. Bell, No. 186,787, January 30th, 1877. The invention, as I understand the specification, consists chiefly in the substitution for the stretched membrane and armature of 'an iron or steel plate capable of being thrown into vibration by sounds made in its neighbourhood.'

"This modification conforms exactly to the telephone receiver which I used in 1869-70, that is, the combination with an electro-magnet of a plate of iron which can be thrown into vibration by the movement of surrounding air or by the attraction of a magnet." Fig. 11 and fig. 12 in the REVIEW show two forms of this receiver.

The deponent further says: "I notice that fig. 5 of the same patent is a drawing of a compound permanent magnet to the poles of which are affixed pole pieces of soft iron surrounded by helices of insulated wire, and that placing helices or coils on the end or ends of the poles of a magnet is claimed as a novelty in this patent."

"I know that placing coils upon the end or ends of the poles of a magnet is not original with Professor Bell. This magnet was described by me in the December number of the *Journal of the Franklin Institute* for 1861, where the illustration of it is found on page 420, and the description of its use for the production of currents identical to those obtained by Bell with his magneto-transmitter."

It follows that Morgan Brown's (Bell's) claim to the use of magnets with coils on the pole pieces is not valid because of the publication of such an instrument used for the same purposes* as Bell's in England in 1862—in the *Journal of the Franklin Institute* for 1861—and was on public view in the library of the British Patent Office in 1862, and is there at present.

I send you the official publication by the U. S. Patent Office of Bell's specification and drawings of his patent of January 30th, 1877, referred to Prof. Van der Weyde in his affidavits.

I send you a photograph representing another view of the instrument exhibited in your drawing No. 11, from which can be seen more clearly the identity of Bell's receiver as shown in his English patent, fig. 20, "a plate of thin steel attached to a frame," with that of Van der Weyde of 1869.

In my letter published in the REVIEW of 23rd February, 1884, I proved by Bell's own writings that he was well acquainted with Philip Reis's telephone instruments, and that he was not an *inventor* of a telephone, but a mere adaptor and improver of the Reis instruments.

Can anyone possessed of ordinary common sense believe that Prof. Bell was not well acquainted with the instruments used by Van der Weyde at a public lecture, and described in a daily newspaper, especially as at that time Prof. Bell says he was greatly interested in his studies in regard to the transmission of words by electricity.

W. C. Barney.

November 3rd, 1884.

[* We fear this statement may mislead. The publication mentioned contains descriptions of four new magneto-electric machines, and the one more particularly referred to was intended to prove the correctness of the principle that the coil may be advantageously wound round the steel magnets. It consists of a permanent horseshoe magnet with a coil on either pole, in front of which is a revolving armature, in shape like the customary keeper. There is no reference whatever to the machine as a transmitter of sound, whether articulate or otherwise, and we do not see that it could be considered in any way as invalidating the Bell patent.

The photograph sent by Mr. Barney represents a perspective view of the instrument, fig. 11, illustrated in our last week's issue. We do not think it shows the vital portions of the apparatus so clearly as that already published. The drawing of the Bell instrument to which Mr. Barney alludes can be found in the ELECTRICAL REVIEW for February 25th, 1882, page 146.—EDS. ELEC. REV.]

Information Required.

Two dynamos, A and B, and an electro-dynamometer are joined in one circuit. If A is put in motion by a steam engine, the currents generated in A will cause the dynamometer to deviate, and the dynamo, B, to rotate. If the movement of A is accelerated, B, too, will run faster.

I think I have read somewhere that in that case *the readings of the dynamometer still remain the same,* whatever the speed of the dynamos may be, owing, of course, to the counter-electromotive force generated in B. Can you, or any of your readers, tell me where I can have found this, and where I can find the mathematical or experimental proof?

T. W. G.

Delft, *November 1st,* 1884.

Duplex and Quadruplex Telegraphy.

Your correspondent "Electricista," in the postscript to his letter contained in your last issue, asks whether the manufacture for sale of sets of duplex telegraphs on either Bridge or differential systems for *educational purposes* or otherwise would constitute an infringement of any patent controlling duplex telegraphy? I regret that I am unable to give a full answer to his inquiry. I may mention, however, that the only cases in which the manufacture of a patented article does *not* constitute an infringement of a patent is when the article is made simply as an amusement or as a model, or for the purpose of *bonâ fide* experiment or where no commercial profit is derived from the use thereof, all of which cases, I apprehend, are without the pale of your correspondent's inquiry.

Thos. J. Handford.

42, Southampton Buildings,
November 3rd.

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